HEROES OF SCIENCE.

ASTRONOMERS.

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Δεῖ δ' ἐλευθέρον εἶναι τῇ γνώμῃ τὸν μέλλοντα φιλοσοφεῖν.

PTOLEMY.

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PREFACE.

THE primary object of this little book is, as its name implies, to give some account of the lives of the chief Astronomers. But it is impossible to leave in the mind of the general reader any clear notion of their characters without giving some account of their work. A good deal of space is therefore taken up with explanations of their discoveries. But as this is only of secondary importance the explanations are given in a popular manner, and no mathematics is introduced, except in ten pages (172–182) where a knowledge of the first book of Euclid and of the elements of Algebra is assumed.

The book may possibly be useful as an introduction to the study of Astronomy, and in this aspect of it it is hoped that it may be helpful in two respects. First, by putting before the student the personal difficulties which the first investigators of the law of the stars met with, and the struggles they passed through to overcome them, whereby a human interest is given to the study of their work; and secondly, by clearly indicating the nature of the problems to be solved by the science.

Dr. Whewell's maxim, that "man is prone to become a deductive reasoner," is unfortunately illustrated in almost all modern books on natural philosophy, where the laws of nature are stated first, and then the phenomena which we observe are deduced from them. The actual process by which our knowledge of nature has been obtained is the inverse of this; and it seems that, by following the historical order, the reasoning is rendered clearer, because its object is seen from the beginning of the investigation, and the study of the science is rendered more interesting.

In attempting to give a connected sketch of the history of so vast a science in so short a space, some of the demonstrations are necessarily condensed; it may, therefore, be convenient to indicate, out of the multitude of elementary works on astronomy, two where the student will find fuller and more elaborate demonstrations of the points only briefly referred to here. These are Sir G. B. Airy's "Popular Astronomy," and his article "Gravitation" in the "Penny Cyclopædia."

It may be of use to enumerate the chief English works on the lives of the astronomers, all of which have been used in this book. They are Whewell's "History of the Inductive Sciences;" Grant's "History of Physical Astronomy;" Martin's "Biographia Philosophica;" Brewster's "Martyrs of Science," and his "Memoirs of Sir Isaac Newton;" the lives of Kepler and Galileo, in the "Library of Useful Knowledge," the former of which, though very short, is one of the most perfect works of its kind to be met with; Mrs. Sturge's translation of Gebler's "Galileo Galilei and the Roman Curia," Haskins's translation of Fourier's "Historical Eulogy of Laplace;" Holden's "Life and Works of Sir William Herschel," the "Memoir and Correspondence of Caroline Herschel," and Smyth, Powell, and Grant's translation of Arago's "Biographies of Distinguished Scientific Men."

But the student who wishes to explore completely the science of astronomy should never forget Sir John Herschel's warning, in the introduction to his "Outlines of Astronomy," that there is but one key that can unlock its mysteries, and that is, "sound and sufficient knowledge of mathematics, the great instrument of all exact inquiry, without which no man can ever make such advances in this or any other of the higher departments of science as can entitle him to form an independent opinion on any subject of discussion within their range."

E. J. C. M.

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CONTENTS.

HEROES OF SCIENCE.

CHAPTER I.

ON ANCIENT ASTRONOMY.

BEFORE the dawn of even the rudest civilization men must have been conscious of the alternation of light and darkness, and were probably aware that the sun appeared to rise, move across the sky, and set during every day. The recurrence of the seasons must have been noticed in the earliest beginnings of civilization; the first farmers and the first sailors must have observed thus much of nature; and in countries where the sky is clear, certain groups of bright stars, maintaining the same relative positions from night to night, and from year to year—certain of the more conspicuous constellations, must have been recognized and probably named; and it required no very great intelligence or labour to notice further that these constellations rose, moved across the sky, and set like the sun, and that different constellations rose at sunset at different seasons, but that, whenever the same season came round, the same constellation rose as soon as the sun had gone down.

But the man who first attempted to classify the stars, or systematically to observe and chronicle astronomical phenomena; who, amongst a race, every individual of which had hitherto lived on, eating and drinking and fighting with his fellows, incurious of the world around him, first felt that there was a secret in nature to be unravelled, and for the mere love of knowledge laboured to unravel it,—was the unknown and unknowing founder of science, the leader of the advance of the human mind. Who this first hero of science was we shall never know. But he, perhaps, is more worthy of our reverence than any. And when he had lived the progress of astronomy was sure.

The growth of civilization would require some method of chronology, and hence the fixation of units or a unit of time. For domestic purposes the day, a period of consecutive light and darkness, would naturally suggest itself; but for political purposes a larger unit would be more convenient, and the period of a complete succession of the seasons, or a "tropical year," would be the most reasonable to choose. In order that these two units might be used together, it would be necessary to determine the number of days in a year.

If there were no other method of measuring the year than by the recurrence of the seasons, this would be an extremely difficult thing to do, and would involve the counting of all the days in a vast succession of years to obtain any degree of accuracy; but the elements of astronomical knowledge solved the difficulty.

It must have been one of the earliest discoveries of the student of nature that the day is longer and the night shorter in summer than in winter. The stars all appear to keep at the same distances from one another, but yet to move each at a uniform rate in one direction, with the exception of the pole-star, which seems to remain fixed in the heavens. Hence all the rest appear to describe circles, at a uniform rate, about the pole-star. The pole-star not being in the zenith or on the horizon, it follows that the stars whose distances from it are less than its distance from the horizon—or "circumpolar stars," as they are called—never set, and of the rest those that are most remote from him remain longest below the horizon. Now, although the sun moves round the pole-star every day in nearly the same time as the stars, he does not keep at the same distance from the pole at all times of the year. This must early have been found to be the cause of the varying duration of day and night. The day on which the sun was nearest to, or that on which he was furthest from the pole, or, which is the same thing, the day on which his shadow at noon, cast by some known object, was shortest or that on which it was longest—respectively the summer and winter solstice—could easily have been observed, and so the duration between any two winter or summer solstices, that is, the length of the tropical year, would have been discovered.

The most conspicuous object in the sky, next to the sun, is undoubtedly the moon, and her strange changes in form—her "phases," as they are called—must early have attracted attention. The period during which she goes through a complete cycle of these changes—between twenty-nine and thirty days—was the origin of that third unit of time, which is called the month.

It was soon found that there was no exact number of days in a month, or of days or months in a year, and various artifices were resorted to to overcome this difficulty. Among all these artifices, one only, that of the Arabs, neglected the sun, and made their year depend solely on the moon; and it is interesting to remember in connection with this that the Arabs practised neither agriculture nor navigation.*

The origin of the grouping of the stars into the particular constellations by which we now recognize them, is lost in the remotest antiquity; but the arbitrary manner in which outlines of men and animals are scribbled over the sky, without any regard to the configurations of the stars which fall within them, suggests a mythological rather than

* *Vide* Whewell's "History of the Inductive Sciences," i. 125.

a scientific origin. It is, however, a puzzling fact that, among all the ancient nations, the constellations were nearly the same.

This amount of elementary astronomical knowledge was held by several of the ancient nations—the Chaldeans, the Egyptians, the Hindoos, the Chinese, and even the Mexicans, and as it is scarcely possible that all these could have derived it from one source, it is probable that astronomical science was originated independently by more than one hero. As, however, we are only concerned with ancient astronomy in so far as it has influenced the discoverers of our present knowledge, we shall only notice here the astronomy of the Chaldeans, and then pass on to consider that of the Greeks.

The Chaldeans were patient and laborious observers, and the clear sky beneath which they lived gave them ample facilities for exercising their powers on the heavens. They therefore advanced astronomical science as far as it was possible without considering the explanations of the phenomena, and there they stopped. Of the men who made their discoveries, or even of the dates at which they were made, we know nothing; but they invented the elements of a calendar, they correctly determined the apparent paths among the stars of the sun, moon, and planets, and they discovered the first complex cycle of recurrence of any astronomical phenomena in their period of eighteen years, which they called Saros.

They discovered that the tropical year consisted of about $365\frac{1}{4}$ days, and so they are said to have made their year consist of 365 days, and then, in order to keep it in agreement with the sun, to have added a month of thirty days to every one hundred and twentieth year; this is an early instance of the method of "intercallation" for such a purpose. The year thus defined for public convenience as an exact number of days, not always the same for each, is called the "civil year." Before, however, this can be of any use, some moment must be chosen from which to reckon time; the Chaldeans chose for this purpose the era of Nabonassar, B.C. 749.

The fact that different constellations rise at sunset at different seasons of the year, and that each constellation appears further and further across the sky as the sun sets day after day until it loses itself in his light, and then after a few days reappears before him in the morning, must have indicated to the Chaldeans that the motion of the sun relatively to the stars is not merely in a line towards or from the pole, but that his path is continuous, completing a circuit among the stars in the course of a year. Later on, the Greeks found that this path, or rather the path of the sun's centre—which they called the "ecliptic," because eclipses only take place when the moon is on it—was a circle which divided the celestial sphere, on which they supposed the stars to be fixed, into two equal parts. Such a line is called a "great circle" of the sphere.

It was much easier to observe that the moon does not maintain the same position relatively to the stars, but that she moves among them in the same direction but at a quicker rate than the sun, in a great circle which she completes in rather more than $27\frac{1}{4}$ days. The position of this great circle is nearly but not quite the same as that of the ecliptic, but is not fixed among the stars, its "nodes," or the points in which it cuts the ecliptic, moving slowly in the opposite direction to that of the sun's annual motion.

Besides the sun and moon, there are five other heavenly bodies, apparently stars, visible to the unassisted eye, which do not retain their positions relatively to the others which are called "fixed stars." It is said that the Chaldeans assiduously observed from the top of their Temple of Belus the motions of these "planets," as they are called. Their apparent motions among the stars are irregular. They will move forward for a time, then stop, turn, and move backwards nearly along the course they have come, stop again, move forwards with a long sweep nearly along their former path, past the place where they formerly stayed, having made a flat loop in the sky; the loops of the path of each planet are sometimes turned to one side, sometimes to the other, of its general course, and the directions in which it turns at the two "stations," or points at which it rests in its loop, are sometimes opposite, so that it appears to describe a flat figure of S. As the forward sweeps are always longer than the backward ones, all the planets do, on the whole, progress among the stars in the same direction as the sun and moon, and complete paths in the sky which never deviate very far from a great circle, and which are always in the neighbourhood of the ecliptic. The path is different for each planet, and the times taken by them to get round among the stars to the same places again, though the same for each planet on different occasions, vary for different planets from one to thirty years. It follows, therefore, that a not very broad band may be conceived to be drawn round the sky, in the general direction of a great circle, which will include the paths of the sun, moon, and planets. This band is called the "Zodiac," and is divided into twelve constellations, which are called the "signs of the Zodiac."

The irregularities in the motions of the planets, watched with the childlike wonder with which the ancient nations must have regarded the heavens, gave rise to the notion that they had some relation to the varying course of human life, and hence arose the belief in astrology. One other fact may be traced to observations on the planets. As well as the day, month, and year, many ancient nations besides the Israelites used a fourth unit of the time —the week; and the names of the days which compose it leave no doubt that the period of seven days was chosen because there were seven heavenly bodies—the sun, moon, and five planets, which change their positions relatively to the fixed stars.

But the greatest debt we owe to the Chaldeans is their careful observations on eclipses, and the consequent discovery of their cycle of 223 lunations, or about eighteen years. Accustomed as they were to the regular succession of astronomical phenomena, an eclipse of the moon, and still more of the sun, must have been to the ancients a strange, and even terrifying event, and it was perhaps natural that they should have carefully chronicled their moments of occurrence. One of these observations, an eclipse of the moon observed at Babylon, in the year 721 B.C., while Hezekiah was reigning in Judah, has become famous in the history of astronomy, and we shall have to refer to it again, in treating of the life of Laplace.* The collection of observations of eclipses, extending over a large number of years, revealed the fact that eclipses of the moon recur in periods of 223 lunations, or about $6585\frac{1}{3}$ days. Hence they could be predicted. Eclipses of the sun are more difficult to calculate, as they depend on the observer's position on the earth. It might have been expected that this discovery of the period Saros, as they called it; of a regularity in apparent irregularity, of law where everything appeared caprice, would have been the death-blow to astrology, and have stimulated scientific research into the explanation of phenomena; but the Chaldeans were, after all, somewhat stupid, and it was not until their more quick-witted and curious followers, the Greeks, took up the study

* *Vide* p. 261.

of nature, that any real advance was made in astronomy.

Tradition asserts that the Greeks owed the elements of their science to the Egyptians. But this is almost certainly incorrect, and whatever knowledge of astronomy they gained from foreigners was probably received from the Chaldeans. It is certain, however, that they rediscovered many of the facts we have already stated.

Thales of Miletus (born B.C. 636) is universally acknowledged to have been the father of Greek science. With his work we come for the first time upon an entirely different aspect of the study of nature—an attempt, rough, no doubt, and grotesque, to reach the *causes* of things. He saw around him the whole physical world in process of change; there must be something *eternal*: amid the growth and decay of existence, he sought *existence* itself. The great question he put was—What is the *beginning* of things? It was everything to have put that question. It matters little that the result of his meditations on it was that *moisture* was the essential principle of the universe.

Tradition ascribes various astronomical discoveries to him. He is said to have measured the apparent width of the sun, to have discovered the "doctrine of the sphere," to have found that the seasons were not all equal in length, and to have believed that the earth was a flat plane floating in water.

Anaximander of Miletus (born B.C. 610), a pupil of Thales, is said to have made the first model of the sky, the first celestial globe, and to have discovered the obliquity of the ecliptic, the true explanation of the phases of the moon, and the spherical form of the earth.

It seems pretty certain that the inequality in the length of the seasons, and the spherical form of the earth, were later discoveries; but the rest may possibly be truly ascribed.

As soon as ever man sought an explanation of the phenomena of the sky, the fact that the stars keep always at the same relative distances from one another, and that the pole-star remains stationary, together with the vault-like appearance of the heavens, would suggest the notion that the stars were all fixed to a sphere which turned once every day about an axis passing through the pole-star and the observer. The horizon, or line along which the earth seems to meet the sky, as seen by an observer on an open plain, seems to be a "great circle" of this "celestial sphere." The great circle, every point of which is equidistant from the pole, and whose plane is therefore at right angles to the axis of the celestial sphere, is called the "equator." The movements of the sun to and from the pole in the course of the year is explained by the fact that the ecliptic is inclined to the equator at an angle of about 23½ degrees. It is an obvious fact that all great circles of a sphere bisect each other at their two points of intersection. Hence when the sun is on the equator just half his course is above the horizon, and hence, since the celestial sphere appears to rotate at a uniform rate, day and night are of equal length. The moments at which this takes place are called the "equinoxes," and the points at which the ecliptic cuts the equator, the "equinoxial points." The points of the ecliptic nearest and farthest from the pole are called the "solstitial points." Hence the equinoxial and solstitial points divide the ecliptic into four equal parts, and the times taken by the sun to describe these parts are called the seasons.

The phases of the moon were probably the astronomical phenomena whose cause was first discovered. The fact that the convex part of the crescent moon, and her full side when gibbous is always turned directly towards the sun, would suggest that she is a spherical body, nearer to the earth than the sun is, and receiving all her light from him. This would be confirmed by observing that when the moon is in conjunction with the sun she is always new, when in opposition to him she is always full, and further, that if when the sun and moon are both above the horizon, a sphere be held at any considerable distance from the observer, in the direction of the moon, the part illuminated by the sun will appear to be exactly of the shape of the moon at the same instant.

Anaxagoras of Clazomenæ (born B.C. 499) first explained that an eclipse of the moon was caused by the shadow of the earth cast by the sun. It is curious, however, to notice as illustrating how confused men's ideas were in the early days of science, that he nevertheless did not believe in the spherical form of the earth. The fact that the sun and moon are always at the same point of the celestial sphere, or "in conjunction," during an eclipse of the sun, and at opposite points, or "in opposition" during an eclipse of the moon, easily suggested the true cause of both. Anaxagoras is interesting as being the first martyr of science. He was accused of impiety at Athens for teaching that the moon, then regarded, with the other heavenly bodies, as divine, was of the same nature as the earth, traversed by hills and valleys, and probably inhabited. He was defended by his pupil Pericles, and escaped with his life; but when an old man of seventy he was cast into prison as an atheist. His friends seem to have understood his work, and his zeal to find out truth, as little as did his enemies, for on his tomb they recorded that "he carried astronomy to its farthest possible limit." The persecution of Anaxagoras was soon avenged. Fifteen years later, Athens was engaged in a life or death struggle. Her forces were divided, and could she only concentrate them victory was probable, otherwise defeat was certain. The largest portion of her forces was at Syracuse. Just as these were leaving, on the 27th of August, B.C. 413, an eclipse of the moon began. The army was in terror. The astrologers commanded a halt of thrice nine days. The one teacher who could have saved his country had been persecuted by her and was dead. The old fallacies were followed, and the power of Athens was destroyed for ever.

As soon as any mechanical contrivance for measuring time was invented, the moments of the equinoxes could be observed. This appears to have been first done by Euctemon and Meton, who worked together towards the end of the fifth century, B.C. Comparing the moments of the equinoxes with those of the solstices, it was found that the sun does not move uniformly in the ecliptic, but that he progresses among the stars rather faster in winter than in summer.

It is certain that by the end of the fifth century men had got to believe in the spherical form of the earth, but as Aristotle (born B.C. 360) was the first whom we know to have given any right reasons for this fact he is sometimes regarded as its discoverer. These reasons are that as the observer travels southwards the pole moves nearer to the horizon and stars appear for a short time above the horizon to the south which never rise to his former northward position; that the shadow of the earth on the moon during an eclipse is always bounded by an arc of a circle, and as the sun and moon are carried round a good way in their diurnal motion in the course of an eclipse, different parts of the earth must cast the shadow, which still remains circular. Hence the earth must be a sphere. This belief was afterwards confirmed by other observations, as that vessels are seen "hull down" at sea, etc.

The earth, then, being a sphere, the horizon still appears to be a great circle of the heavens at all points of her surface, and however far an observer travels in one direction on the earth's surface, the distance between any two stars seen in that direction never seems to increase. These facts can only be explained by supposing any distance that can be traversed over the earth is infinitely small compared with the distance of the stars, so that the whole earth must be a mere point at the centre of the celestial sphere; and hence the lines drawn from any number of points on it towards any one fixed star may be considered as practically parallel.

This principle was used by Eratosthenes (B.C. 276–196) to measure the earth. A line drawn due south or north from any point, and continued round the earth, would be a great circle of the earth, and is called the "terrestrial meridian" through that point. Hence, any arc of it would be the same part of the whole as the angle subtended by that arc at the centre is of the sum of all the angles that can be fitted in round a point in one plane, that is, four right angles or 360 degrees. Eratosthenes supposed that the sun as well as the fixed stars is infinitely distant from the earth. And then it is capable of mathematical proof from the principle referred to above, that the angle subtended by an arc of the meridian at the centre is the same as the difference between the heights of the sun's centre at noon as seen from its extremities. Eratosthenes thought that Syene, in Upper Egypt, was due south of Alexandria; he measured the distance between them and found it 5000 stadia. The sun at the summer tropic was in the zenith at noon in Syene; on the same day he was $7\frac{1}{2}$ degrees from the zenith at Alexandria. Hence the astronomer had to solve the rule of three sum, as $7\frac{1}{2}$ degrees is to 360 degrees, so is 5000 stadia to the earth's circumference; whence the earth's circumference must be 250,000 stadia, or rather more than 30,000 miles.

The followers of Pythagoras were among the earliest Greek astronomers. It is said that their master first taught that the morning and evening star are the same planet (Venus), and Philolaus, a Pythagorean, about the end of the fifth century wrote a work which has become famous. In this, which is a strange mixture of metaphysics and astronomy, he argues that the centre is the most honourable place, that fire is the fundamental principle of the universe, and hence that all nature must revolve round a central fire, which it is important to observe is not the sun. The sun, moon, five planets, and the earth, with an additional body called the antichthon, put in to satisfy some preconceived notions as to the number of the heavenly bodies, revolve round this central fire, the whole being surrounded by the rotating sphere of the sky. This theory, vague as it is, and even inconsistent with itself, is the first that assigned any motion to the earth.

Another Pythagorean, Hicetas of Syracuse, about the same time rejected this theory, but showed that the apparent diurnal rotation of the heavens might be produced by the real rotation of the earth, and appears to have thought this the more likely explanation.

It is, perhaps, worthy of notice, although the fact had no influence upon the history of modern astronomy, that Aristarchus of Samos, at the beginning of the third century, B.C., held that the apparent diurnal motion of the heavens was due to the real diurnal rotation of the earth on her axis, and that the apparent annual motion of the sun among the stars was due to the real annual revolution of the earth about him. No positive reasons for this belief have been left by Aristarchus. He is interesting, however, as being the first to solve a trigonometrical problem in astronomy. He knew that if any two angles of a triangle are known, its shape is determined; and he knew that when the moon appears "half moon," the lines joining her centre to that of the sun, and to the observer, must be at right angles. At the instant of half moon he observed the angular distance of the sun and moon apart, then he knew two angles of the triangle formed by joining the centres of the sun and moon with each other and with the observer. Hence he could calculate the relative distances of the observer from the sun and moon.

There are three sources of error in this process. It is extremely difficult to determine the instant of half moon; the sun's edge is no distinct sharp line, and so it is difficult to determine his exact position; and the distance of the sun being much greater than that of the moon, a small error in their observed angular distance apart would make a large error in their relative distances from the observer. Hence it is not surprising that Aristarchus found the sun to be eighteen times as far off the earth as the moon is, while the fact is that he is four hundred times the distance.

About the beginning of the fifth century, B.C., it seems to have been the fashion to invent cosmogonies. The celestial sphere was supposed to bound the universe, inside this were seven spheres concentric with it, each of five of them carrying a planet, one carrying the sun, and the remaining one the moon; inside the whole was the sphere of the earth concentric with the rest. This spherical universe, with the uniform motion of its outer sphere, seems to have suggested that the natural state of motion of a body was motion at a uniform rate in a circle; and hence Plato (born B.C. 427) is said to have strongly urged his countrymen to explain the complicated motions of the planets by some combination of uniform circular motions—"to reconcile the celestial phenomena by the combination of equable circular motions.*

This challenge was first taken up by Eudoxus of Cnidus, about the year 366 B.C. It was obvious that the general character of the motions of the

* Quoted in Whewell's "History of the Inductive Sciences," i. 178.

planets might be explained by supposing each to be fixed to the rim of a wheel which revolved, turned edgeways to the earth, while its centre was carried round the earth. Such a wheel would carry the planet sometimes backwards, sometimes forwards, but on the whole would cause it to progress through the sky. The mechanism which Eudoxus imagined was as follows. He supposed the celestial sphere to rotate round the earth, each planet to have one sphere rotating with the celestial sphere or "primum mobile," as it was called later on, and producing the diurnal motion. Inside this, concentric with it, and joined on to it by its poles, with its axis perpendicular to the plane of the ecliptic, another sphere slowly rotated, giving the planet its motion along the ecliptic. In this and near its equator were attached the poles of a small sphere with its axis also perpendicular to the ecliptic; this revolving faster than the last produced the stations and retrogressions of the planets. In the equator of this were placed the poles of a very small sphere, which therefore produced the small deviations of the planet from the ecliptic; in the equator of this sphere the planet was fixed. There were three spheres to each of the sun and moon, the explanations of which are obscure. Eudoxus was a most devoted labourer in the cause of science. He appears to have been the first to condemn the belief in astrology, and he used to say that he would willingly suffer the fate of Phaethon, provided he could approach within such a distance of the sun as would enable him to discover its figure and magnitude.*

Callippus of Cyzicus pointed out that certain minute irregularities in the motions of the planets could not be explained by Eudoxus's system, and in order to correct it, he introduced various other small spheres, raising the total number to fifty-five.

Since all these motions were supposed to take place in the equators of their several spheres, it is obvious that they might be accounted for by a mechanism of bars, each revolving in one plane. The first bar being supposed to have one end at the centre of the earth, and to revolve about it in the plane of the ecliptic, at its other end was attached a second bar, which revolved at a greater rate in the same plane, producing the stations and retrogressions of the planet. These two together produced the motion in the ecliptic or the "motion in longitude." At the other end of the second bar was attached a third, which revolved in a plane at right angles to that of the ecliptic, and produced the motion out of the ecliptic or the "motion in latitude." The other end of this bar carried the planet. Such an arrangement, producing a motion, which might be called that of circles upon circles, or "epicycles," was substituted for the spheres of Eudoxus, by Apollonius of Perga (B.C. 220), who first investigated the sections of the cone, and who was therefore called the "great geometer."

* Quoted from Plutarch in Lewis's "Astronomy of the Ancients," p. 147.

We have now to consider the work of incomparably the greatest hero of ancient science, Hipparchus of Alexandria, whose life may be placed between the years 170 and 120, B.C. His first great triumph was the explanation of the apparent motions of the sun and moon. These * might be reconciled with the assumption that the natural state of motion of a body was that of uniform motion in a circle, by supposing that they each moved at a uniform rate in an "eccentric" or circle whose centre did not coincide with that of the earth; then, when the sun or moon was moving through that part of their course which was nearest to the earth, it would appear to move faster than when moving through that which was more remote.

But Hipparchus differed from all previous men of science in not being content with a mere general qualitative account of the sort of mechanism that would produce the sort of motions observed; but he proceeded further to examine quantitatively what the relative magnitudes of the parts of the mechanism must be, and to find their actual position at some particular instant of time or "epoch," in order to bring the sun, moon, or planet, into its observed position at each moment. "Having ascertained," says his successor Ptolemy, "that the time from the vernal equinox to the summer solstice is 94½ days, and the time from the summer solstice to the autumnal equinox 92½ days, from these phenomena alone he demonstrated that the straight line

* *Vide* pp. 6, 7, 14, above.

joining the centre of the sun's eccentric path with the centre of the Zodiac (the observer's eye) is nearly the twenty-sixth part of the radius of the eccentric path, and that its apogee (the point where it is farthest from the earth) precedes the summer solstice by 24½ degrees nearly."* Having ascertained this he was able to construct solar tables giving the quantity (the "prosthapheresis") which must be added to or subtracted from the position in which the sun would be if he appeared to move at a uniform rate among the stars in order to obtain his actual apparent position at any instant.

A similar method served to determine the exact position of the lunar eccentric, and the two together enabled him to calculate and predict solar and lunar eclipses, and so to verify with great exactness the truth of his determinations.

The apparent motion of the moon is very complex. Having performed her course among the stars, she does not return as the sun does, to exactly the point from which she started, the period of her restoration in longitude not being the same as that of her restoration in latitude; her motion may be expressed by saying that her nodes regress.† But Hipparchus discovered that not only is this the case, but that her apogee is not stationary as he thought the sun's was, but that it advances in the direction in which she moves among the stars, so

* Quoted in Whewell's "History of the Inductive Sciences," i. 185.

† *Vide* p. 7, above.

that her eccentric orbit must be conceived as turning round in its own plane. This he discovered by Chaldean records of three eclipses observed at Babylon in the years 384 and 383 B.C., and by Greek records of three others observed at Alexandria in the year 202 B.C.

In the same way Hipparchus ascertained the relative sizes of the epicycles of the planets, but he found certain minute irregularities which he was not able to account for by putting on more epicycles, owing to the want of exact observations of the positions of the planets at different times. Accordingly he laboured to make such observations himself, and left for the use of his successors more that were made by himself than the whole number he had received from all preceding ages.

But perhaps his most interesting discovery is that of the "precession of the equinoxes," revealing as it does not so much genius, which must always be the prerogative of the few, as careful and honest labour which is possible to all. Examining the position of the ecliptic among the stars, by noticing where the moon was when eclipsed, and knowing that the sun must then be at the exactly opposite point of the celestial sphere, he found that the bright star Spica Virginis was six degrees behind the autumnal equinoxial point, whereas, according to the observations of Timocharis, an Alexandrine astronomer, who made a catalogue of the stars between the years 293 and 272, B.C., it was eight degrees behind; observing the place of Regulus and other bright stars, he found a corresponding change of position. Hence the equinoxial points must be moving among the stars.

The question then arose, Was the equator fixed among the stars, and the ecliptic revolving on it? or was the ecliptic fixed, and the equator moving? or were both moving? By a series of the most careful observations on the position of the stars with respect to the equator and also to the ecliptic, he ascertained by comparing them with the observations of Timocharis that the ecliptic was fixed, that the equator continually cut the ecliptic at the same angle, but that the equinoxial points were slowly advancing along the ecliptic so as to complete a circuit in about 25,000 years. It follows from this that the period of the succession of the seasons is not exactly the same as that of the revolution of the sun among the stars; the former is called the tropical year, the latter the siderial year.

When we consider the extreme minuteness of this motion, and the rough instruments which Hipparchus had to work with, we may indeed exclaim with Ptolemy, truly "he was a most truth-loving and labour-loving man."

About 250 years after Hipparchus, Ptolemy studied the heavens at Alexandria. His chief work was entitled "Megiste Syntaxis" ("The Great Construction"), and was hence called by the Arabians, who preserved it for us, Al Magisti. This word has been corrupted into the Almagest, by which name the work is now generally known. In the third book Ptolemy proves that a mechanism of two bars, the first of which revolves about one end in one plane, having the second attached to the other end, and revolving in the same plane, and at the same rate but in the opposite direction carrying the sun at its extremity, would cause the sun to move in a circle eccentric with respect to the fixed end of the first bar; and hence an arrangement of epicycles instead of eccentrics would account for the motions of the sun and moon as well as of the planets.

But it was in attempting to verify Hipparchus's theory, by observing the angular distances from one another of the sun and moon at successive instants, and comparing them with those calculated from the theory, that Ptolemy made his most celebrated discovery. Speaking of the observed and calculated distances, he says, "These sometimes agreed and sometimes disagreed;" but by further examination he found a law or order in their disagreement, for he continues, "As my knowledge became more complete and more connected, so as to show the order of this new inequality, I perceived that this difference was small or nothing at new and full moon; and that at both the dichotomies (when the moon is half illuminated), it was small or nothing, if the moon was at the apogee or perigee of the epicycles, and was greatest when she was in the middle of the interval, and therefore when the first inequality was greatest also." * The first inequality from uniform motion in a circle explained by the eccentric of Hipparchus is usually called the "equation of the centre." This inequality, discovered by Ptolemy, is called the "evection," and its greatest value is found to be about $1\frac{1}{3}°$. And with great difficulty Ptolemy succeeded in accounting for it by supposing an epicycle to carry the moon, and to move round the eccentric of Hipparchus, or by putting on a third bar to the mechanism explained in the third book of the Almagest.

Ptolemy also ascertained from the observations of Hipparchus what the epochs and relative magnitudes of all the epicycles of each planet must be, and he was led to the conclusion that the centre of that epicycle of each planet which produces its stations and retrogressions must move, not in a circle concentric with the earth, but in an eccentric; and, further, that it does not move at a uniform rate, but in such a manner that the line joining it to another point in the plane of its motion, called the equant, and which is neither the centre of its eccentric path nor that of the earth, may revolve at a uniform rate. The equant was supposed to be in such a position that the line joining it to the centre of the earth was bisected by the centre of the eccentric. Lastly, in order to explain how it is that the progressions and retrogressions of each planet do not take place along exactly the same

* Quoted in Whewell's "History of the Inductive Sciences," i. 229, from "Syntaxis," v. 2.

line, but make flat loops in the sky, which are turned sometimes to one side, sometimes to the other of their general direction, it was supposed that the plane of the great epicycle of each planet was slowly tilting or "librating," so that an observer on the earth would see sometimes one side of it and sometimes the other.

In this complicated state the knowledge of astronomy—of the law of the stars—was left by the ancients.

To us who are familiar with Newton's great discovery, it seems strange that men could be content with so confused a theory. Still, the theory, such as it was, introduced a sort of order where everything had seemed anarchic, and hence stimulated the belief in the boundless empire that was destined to be conquered by the mind of man. Seneca, towards the end of the first century after Christ, confidently declaims, "The time will come when those things which are now hidden shall be brought to light by time and persevering diligence. Our posterity will wonder that we could be ignorant of what is so obvious.* But the world had to wait through long ages of darkness for any further light in science.

During the decline of the Roman Empire, the Mohammedan Arabs conquered Egypt, and the best of them eagerly adopted the science of the Alexandrian Greeks, which had been rescued from the

* Quoted in Whewell's "History of the Inductive Sciences," i. 223, from Seneca, "Qu. N." vii. 25.

general ruin they wrought. But like the Chaldeans, they were incapable of adding anything to the theory. Two important discoveries were indeed made by them. A native of Batan, hence called Al Batani, found that the sun's apogee was moving, and that this motion might be explained by supposing his eccentric, like that of the moon to revolve slowly round the earth in its own plane. Hence there is a third kind of year, called the "Anomalistic," which is the period occupied by the sun in returning to the same part of his orbit, in moving, that is, from perigee to perigee, or from apogee to apogee. Later on Aboul Wefa found a third inequality in the moon's motion ; but it illustrates the slavish stupidity of the race, that this discovery was forgotten by them, and it is known to us now from its rediscovery by Tycho Brahé ages after.

Among the nations of Christendom science completely died out during the Middle Ages. The Roman Empire had crushed out all national life, and with it all individual energy. Even the strong individuality of the Christian religion failed to revive it; and had it not failed, it is questionable whether the history of science would have been different. All the intellectual activity of the Greeks had not availed to cleanse—had rather, it seemed, stimulated the frightful moral rottenness of the world, at the time of the coming of Christ; and the early Christians, possessed with the knowledge of the overwhelming importance of man's spiritual progress, regarded philosophy rather with contempt. Eusebius says, "It is not through ignorance of the things admired by them, but through contempt of their useless labours, that we think little of these matters, turning our souls to the exercise of better things." * But, as it was, Christianity itself was slain, or at least enslaved for a time.

The supreme prestige of the eternal city, the long continuance of universal empire, created, when Christianity was established by Constantine, the notion of the absolute authority of a universal Church. Submission to "the Church" supplanted that personal loyalty to the Master, that had strengthened His followers to bear witness to Him in the arena and at the stake. All individual life was crushed out. Men became spiritual weaklings, accepting their religion, not on personal faith, but on "authority;" and as they became spiritual weaklings, they became intellectual weaklings. To launch alone upon what, even to Newton, seemed the infinite ocean of the unknown, appeared to them to be impious. They feared to walk by themselves; they needed to lean on some intellectual superior. They feared to question Nature for themselves, and so they were content to learn the opinions of great thinkers.

Aristotle soon became the supreme authority in all scientific matters. John of Salisbury, writing in the twelfth century, says: "The various masters

* Quoted in Whewell's "History of the Inductive Sciences," i. 269; from "Præp. Ev." xv. 61.

of dialectic shine each with his own peculiar merit, but all are proud to worship the footsteps of Aristotle; so much so, indeed, that the name of philosopher, which belongs to them all, has been pre-eminently appropriated to him." * His works became the Bible of mediæval science, and to question them was heresy. Calippus of Cyzicus had communicated to him the theory of Eudoxus, and this had been adopted by him in his scientific works. His mediæval commentators regarded the work of Hipparchus and Ptolemy as a mere extension of this theory, and so the epicyclical theory came to be regarded as of almost divine authority.

But even the observations of Hipparchus and Ptolemy were not absolutely correct, and so their epicycles did not exactly account for the apparent motions. The error was infinitesimal, but year by year, through the long centuries of mediæval slumber, this little error was accumulating, and the planets were getting farther and farther from their places as described by the theory; and generation after generation passed, each slavishly learning the theory from the preceding one, and it never occurred to a man among them to look up into the sky, and see if these things were so. And even if they had done so, and seen that nature contradicted their theory, they would not have believed it; for their successors cursed and maltreated the first man who proved it to them.

* Quoted in Whewell's "History of the Inductive Sciences," i. 336; from "Metalogicus," ii. 16.

The motto of the Church was "semper eadem," and progress was sin.

At last the morning of freedom—intellectual, spiritual, political—began to break; and the first ray of light that shot across the gloom, the first gleam of originality among those dull worshippers of authority is to be found in the saying of Alphonso X., king of Castile, in the twelfth century, who, when his instructors had explained to him the complicated but orthodox theory of Ptolemy, exclaimed that if he had been consulted at the creation, he would have made the world on a simpler and better plan.

CHAPTER II.

ON COPERNIK AND HIS SYSTEM.

It must have been obvious to our readers, that in the preceding sketch of ancient astronomy, we have been dealing with investigations of nature that differed not only in the degree of energy with which they were pursued, or of success with which they were attended, but also in the kind of method on which they were undertaken. The old Chaldean investigations, consisting simply of observations of phenomena with the consequent unexpected discovery of periods of time, or cycles, in which certain sorts of phenomena recur, were obviously inferior to the attempts of Thales to reach the causes of the phenomena; and the vague and uncertain speculations of Thales, with his confused notion that moisture was the essential principle of the universe, were inferior to the theories of Eudoxus, which gave a reasonable account of the sort of mechanism which would explain the phenomena observed. And these again were obviously inferior to the systems of Hipparchus and Ptolemy, which gave a reasonable account, not only of the sort of mechanism required, but of the actual magnitudes, or relative magnitudes of its parts. And no one who has followed us so far can doubt that the process employed in this last one is the true and fair method of investigating nature. The whole success of modern astronomy has been due to its adoption; it may be well, therefore, to give some account of it here.

It may seem strange that after Hipparchus and his followers had given the world examples of this right process, and had thereby attained such striking success, men should ever have fallen into the word quibblings and hair splitting casuistries of the Middle Ages; but it must be remembered that this right process, though used with success was never formulated, and that false science was followed side by side with true, even in the days of Hipparchus and Ptolemy. It was not until after the Renaissance that the true method was placed in sharp antagonism to the false ones, and it was reserved for Francis Bacon first to formulate the rules of induction.

One other reason must be mentioned. It has been observed by Dr. Whewell * that "man is prone to become a deductive reasoner;" and mathematics was the earliest branch of human knowledge to advance to any considerable extent. The success that was won in that direction may

* "History of the Inductive Sciences," i. 159.

have induced men, when their ideas were still confused and misty, to believe that the method there pursued was applicable to every kind of investigation. In mathematics, men think out certain fundamental principles or "axioms," which are inseparable from the human mind; as that "the whole is greater than its part," that "things which are equal to the same thing are equal to one another," and so on, and then they deduce their necessary consequences, which are stated in propositions; and whether it was generally believed during the Middle Ages that it was possible in the same way to think out the fundamental principles of nature, or not, at any rate the obsequious respect that was paid to the authority of Aristotle, led to the belief that he had somehow ascertained and stated all these fundamental principles, and that all that was needed was fairly to deduce their necessary consequences.

We now know that exactly the opposite course has to be pursued; that the study of nature, instead of resembling that of mathematics, is exactly the inverse of it. In mathematics the human mind has direct contact with the string of argument at the beginning of it, in those principles which it perceives to be necessary truths; but in the study of nature, it is the principles we want to find, their necessary consequences we have in the phenomena of the material universe, and we can observe them; the mind, in this case, has direct contact with the string of cause and effect at the end of it, in the consequences of the principles, which can be directly perceived through the senses; it is therefore our object not to deduce—to lead down—the results from the principles, but to ascend to the principles from the results. This process is much more difficult than the former; one step of it, as we shall see more clearly hereafter, demands the highest effort of genius, for which no rules can be laid down; but the whole process, a complete "induction," as it is called, consists of three steps.

First, all the facts which are to be accounted for must be carefully and exactly observed and recorded.

Secondly, the explanations of these facts must be guessed at, theories must be invented, principles must be assumed, which, if they were true, would have these facts for their necessary consequences. It is here that genius comes in; it is this that links the man of science with the poet; the laborious exactness of the observer is not more necessary to the advancement of science than the soaring imagination of the poet; and the more exuberant, nay, the more wild is his imagination, the more likely is the man of science to arrive at truth, provided always he be honest.

And this leads us to the third step, the verification of our theories, by deducing the further necessary consequences of each in turn, and observing whether these take place or not. Those theories whose necessary consequences do not take place are certainly false; those theories whose necessary consequences do take place *may* be true.

When one theory alone has been discovered which will account for all the facts hitherto observed, and when it has been verified in a vast number of particular instances, we are entitled to believe that it is a true theory, that it correctly states the order according to which phenomena occur. Such a theory is called a "law of nature."

Laws of nature are, therefore, after all, merely our own inventions. They possess no authority, other than that derived from their agreement with the facts they are invented to explain; they are inferior, not superior to phenomena. It is not true, for instance, to say that the planets move in ellipses because the law of gravitation constrains them; what is true is, that we believe the law of gravitation to be true, because the planets move in ellipses. All that laws of nature do is to "colligate," or bind up together in our minds all the facts, to account for which they have been invented.

In the case of astronomy, the observed facts are the apparent motions of the heavenly bodies. The theory, so far as we have yet considered the history of the science, is the theory of epicycles; the verification consists in the construction, on the assumption of the truth of the theory, of tables which predict the positions of the heavenly bodies at particular instants of time.

Ever after the declaration of Alphonso X. with which we closed the last chapter, there was a continuously growing appreciation of the true method of investigating nature, which showed itself, first, in the discovery that the tables that had been left by the ancients were inaccurate, that the actual position of the heavenly bodies at any instant was not at all that which the tables predicted, and hence that the theory could not be perfectly true; and secondly in the attempt so to correct the theory as to construct tables, by means of it, which should accurately predict the motions of the heavenly bodies.

Alphonso himself (born 1226, began to reign 1252, died 1284) was the first to project such a work, and thereby won for himself the title of "the Wise." At his instance the Moorish astronomers of Grenada constructed tables, called after his name the "Alphonsine Tables," which gave the positions of the heavenly bodies from May 30, 1252. These were first printed at Venice in 1492, and were used down to the end of the sixteenth century.

In the year 1460, George Purbach published a corrected theory, on the lines of the epicyclical system of Ptolemy.

John Müller (died 1476), who styled himself Regiomontanus, probably because he was a native of Königsberg, extended those branches of mathematics which have a direct bearing on astronomy, particularly plane and spherical trigonometry, which had been first investigated by Hipparchus.

Werner (died 1528) conducted an elaborate series of observations, which led him to a more correct value of the precession of the equinoxes.

And lastly, Fernel, in the year 1528, following the general principles on which Eratosthenes had worked, discovered more accurately the size of the earth.

During these two centuries and a half in which the reason of men had been slowly awakening, their imagination still slumbered on. For it never occurred to any one, all that while, to seek whether some entirely new system, quite different from the Ptolemaic, would not better account for the facts—unless we except Nicholas of Cusa, a cardinal and bishop, who in a work, "De Doctâ Ignorantiâ," published during the first half of the fifteenth century, demonstrated that the rotation of the earth would explain the apparent diurnal motion of the heavenly bodies, as well as the rotation of the celestial sphere with the whole apparatus of epicyclical mechanism needed to account for the independent motions of the sun, moon, and planets. This he seems to have maintained, however, more as a curious paradox than a reasonable theory. Still we cannot refuse him the credit of giving the first sign that independent imagination still lived in the world of science.

Side by side with this there were evidences that the world was spiritually awakening. Wycliff in England, Savonarola in Italy, were stirred with horror at the gross immorality of the monks, and declaimed against the miserable shams maintained by the Church of Rome. At last an event happened which stimulated learning and resulted in the Renaissance. Constantinople was taken by the Turks, and the classical learning of the sages who had clustered round the throne of the Greek Emperors, was scattered over Western Europe.

This was in 1453. At 4.38 on the 19th of January, 1472, according to Junctinus, or at 4.48 p.m. on the 19th of February, according to Moestlin, at Thorn in Prussia, was born Nicholas Copernik, Koppernigk, or Zepernigk, as the name seems to have been originally—the first of that long line of modern astronomers, who, peering into the secret of the heavens—apparently the most abstruse and difficult, and ambitious effort of human genius, which seemed so far above the power of human intellect, that men thought the attempt was almost profane—have solved it for us with incomparable completeness, and have revealed it to us in the most clear, and certain, and accurate of all the inductive sciences.

Of the life of Copernik, but little has been left to us, probably because little was to be told ; it was singularly quiet and uneventful. Yet we know enough to gain a tolerably vivid notion of the man. He was of humble origin. His father Nicholas was of the Sclavonic race that inhabited Bohemia ; his mother was sister to Lucas Watzelrode, afterwards Bishop of Warmia.

His study of Greek and Latin was begun at home, and continued afterwards at the University of Cracow. Among his fellow-students were some who afterwards became more or less famous, and with one, Martinus Ilkusius, he afterwards corresponded on astronomical subjects, particularly on eclipses. At the University, medicine and philosophy were his chief studies, and he ultimately took the degree of doctor of medicine. But he also attended the lectures, private and public, of the professor of science, Albertus Brudzevius, from whom he learned the use of the cumbrous astronomical instruments then known. It was about this time, according to his eighteenth century biographer,* that he "at length grew so emulous of the illustrious fame of Regiomontanus, that he resolved to follow him in all his steps." The remark is, as we shall see, essentially false, and throws no light whatever on Copernik, though it throws considerable light on Mr. Benjamin Martin and the eighteenth century.

His work now took a more definitely astronomical direction: mathematics, especially perspective, and with this the art of painting, were his chief pursuits; the latter, it is said, was taken up that he might be able to carry away remembrances of his travels in Italy, on which he had now determined to set out. One famous result of his skill was a half-length portrait of himself, which afterwards came into the possession of Tycho Brahé.

After leaving the university, he returned home

* Mr. Benjamin Martin, in his "Biographia Philosophica," p. 164.

for a short time, and then set out for Italy in his twenty-third year. He stayed first at Bologna, where we know him to have been in 1497, in order to study under Dominico Maria, who, according to Mr. Benjamin Martin,* "for twelve years had taught astronomy with great applause." This seems to have been the beginning of his astronomical researches, and while here he is said to have speculated that the height of the pole is not always the same at the same place.

In the year 1500 he went to Rome, and was soon installed with great ceremony as Professor of Mathematics. While there, his thoroughness and clearness, and the accuracy of his astronomical observations, so struck all who met him that men began to couple his name with that of Regiomontanus, as one of the most laborious and learned of modern astronomers.

In a few years, while still quite young, he returned home, having by this time become a priest. His reputation had gone before him, and his uncle, now Bishop of Warmia, received him with affectionate pride, and settled him in the College of Canons at Frauenburg, where was the cathedral of the diocese. Here he remained for the rest of his life, devoting himself to three things in particular —his ecclesiastical duties, the administering of gratuitous medical help to the poor, and, lastly, and chiefly, meditation on the system of the world. For this purpose he loved solitude, and seems to

* Biographia Philosophica, p. 165.

have made but few intimate friendships. It is worthy of note that he altogether avoided disputations, and entirely disbelieved in their utility, at a time when all learned men, and particularly the Aristotelians, were continually engaging in them. Yet it must not be supposed that the quiet, pensive monk was a mere unpractical dreamer or a contemptuous misanthrope. Whatever his hand found to do he did it with his might. Whenever the moment came for speaking he could speak with authority, and his faithfulness gained the respect and even affection of his brother canons.

Lucas Watzelrode was frequently at court on public business, and used to leave the affairs of the diocese in the hands of the young Copernik, and everything was sure to go well. On one occasion some contention arose between the canons and the Teutonic Knights, and Copernik championed the cause of his fellows with complete success. At another time he was unanimously chosen by them as their representative at the Assembly of the States which met at Grodno. The chief question before the Assembly was the confusion in the currency, and the depreciation in the value of money caused by recent wars. A committee of Polish senators was appointed to examine into the matter, but they could resolve on nothing. At last Copernik drew up a canon or rule for computing all the different sorts of money current in the various provinces of the kingdom, and for reducing them to a common standard. This was at once felt to be a complete solution of the difficulty, and the Senate placed it among their public acts.

In 1516, Paul of Middleburg, Bishop of Sempronia, president of the committee appointed by the last Lateran Council to consider the reformation of the calendar, applied amongst others to Copernik for assistance and advice; but the astronomer would do nothing precipitately, and seems to have felt that no help could be given until he had attained to clear and distinct ideas as to the system of the world.

Long before this he had attained to a general notion of that system, and we know the steps by which he advanced to the truth. His first great reason for doubting the Ptolemaic system was its complexity, particularly with respect to the motion of each planet about a separate equant; and Rheticus adds that a minor reason was the great variation in the apparent size of the planet Mars, for which no adequate explanation was given by the epicyclical theory. These difficulties determined him to search through the works of all the ancient astronomers for suggestions of a better theory.

Three suggestions he met with greatly impressed him. First, Martianus Capella, in the fifth century, pointed out that, inasmuch as each of the planets Mercury and Venus always stays and turns in its course among the stars at the same angular distance alternately behind and in front of the sun, the centre of the great epicycle of each must lie in the line joining the observer to the centre of the sun, and possibly may lie at the centre itself. So that the sun might be revolving round among the stars in the course of a year, carrying Mercury and Venus revolving round him at a greater angular rate. Copernik was so struck with the simplicity of this idea that he made the remarkable prediction that "if the sense of sight could ever be rendered sufficiently powerful, we should see phases in Mercury and Venus." Secondly, the theory of the earth's rotation held by Hicetas * seemed to explain the apparent diurnal motion of the heavens more simply than the received theory; and thirdly, the orbital motion ascribed to the earth by Philolaus † suggested the trial whether a modification of it might not lead to a simpler explanation of the independent motions of the sun and planets.

It appears that the main outlines of his system were formulated and held by him as early as 1507, for in that year he writes: "By a close and long observation, I have at length found that, if the motions of the rest of the planets be compared with the circulation of the earth, and be computed for the revolution of each, not only their phenomena will follow, but it will so connect the orders and magnitudes of the planets and all the orbs, and even heaven itself, that nothing in any part of it could be transposed without the confusion of the rest of the parts and of the whole universe."

* *Vide* p. 17. † *Vide* p. 16.

But a mere general view of the sort of system that would explain the apparent motions could not satisfy him. Like Hipparchus, he wanted to find the magnitudes of the orbits and periodic times of the planets, and for this purpose observation was necessary. This Copernik saw clearly, and his right appreciation of its fundamental importance in science so possessed him, that years afterwards he declared to his pupil Rheticus, "If I could determine the true places of the heavenly bodies to within ten seconds of a degree, I should glory not less in this than in the rule Pythagoras has left us." But he possessed no better instruments than Ptolemy. He made a quadrant, which was erected in the plane of the meridian, and by means of the shadow cast by a peg at its centre he could measure the height of the midday sun at solstices and equinoxes, and hence determine the position of the ecliptic. The rest of his observatory consisted of a parallactic instrument of fir wood, resembling a pair of compasses, with a limb divided into 1414 equal parts, and by pointing its two legs, one at one planet and the other at another, he could measure their angular distance apart. With these rude contrivances, however, he attained his end, and it is said that a complete quantitative account of his system was written as early as 1530.

All this while he was careless of, or rather as far as possible avoided, "fame." Yet he was always willing to explain his ideas to any who really wished to learn about the stars. And so, as the years rolled slowly on, and, one by one, men had come and listened to Copernik, and had gone away convinced, perhaps as much by the calm, open-minded honesty of the man as by the intrinsic truth of his reasoning, it came to pass that the name of the great sequestered man was known far beyond the confines of Frauenburg as that of one who had thought out a new and original system. And then, like all who have ever done a new work for the good of men, like all who have enlarged the bounds of human knowledge, he had to suffer the laughter of fools; he was satirized on the stage at Elburz.

Nor could he altogether escape the admiration of the wise. At the University of Wittemberg, George Joachim, commonly known as Rheticus, from being a native of the Northern Tyrol, anciently inhabited by the Rheti, was Professor of Elementary Mathematics. He was only twenty-five years old, but he was an honest-minded, earnest man, and with his friend Schöner, Professor of Mathematics in Nuremberg, was curious to understand the new system. Accordingly he went to Copernik, and was so struck with him, that he gave up his professorship and took up his abode at Frauenburg. In about two months he wrote to his friend, "Believe me, most learned Schöner, that this man, my instructor in all kinds of learning and in the science of astronomy, is not inferior to Regiomontanus. I freely compare him to Ptolemy; not that I think Regiomontanus less than Ptolemy, but because my preceptor has a felicity in common with Ptolemy, that, by the divine assistance, he would finish the reformation of astronomy; whereas Regiomontanus, O cruel fate, died before he had set up his pillars."

This was in 1539. In about two years Rheticus returned to Wittemberg and resumed his professorship, having brought with him a sketch of the Copernikan system. This was the first account of it that was ever published, for Copernik had repeatedly rejected the entreaties of his friends that he would print the complete manuscript that he had had by him so long; for some reason he steadily refused, and continued to correct and improve and polish it in private. At last Cardinal Nicholas Schonberg, Bishop of Capua, who, with Tydemann Gyse, Bishop of Culm, had been urging him ever since 1536, persuaded him to publish the work. Accordingly, in 1541, he gave the manuscript to Gyse, who sent it to Rheticus. Rheticus thought it might be best printed at Nuremberg, and so he handed it over to Andrew Osiander, who superintended the publication. Before we proceed further, it will be well to give some account of this work, which was entitled "De Revolutionibus Orbium Cœlestium."

The formal system of the world which we now hold, and which must be considered to have been left to us in its present completeness by Newton, is generally called the Copernikan system. This leads to confusion, for we owe but a small part of it to Copernik. If, therefore, we would clearly understand what each worker has done, and what difficulties he has conquered, we must consider somewhat carefully the system actually held by Copernik and explained by him in his book.

The work consists of two parts: first a dedication to Paul III., then pope; and secondly the treatise on the system of the world. This latter is divided into six books.

The first of these books again consists of two parts, the second of which we may at once dismiss with the statement that it deals with trigonometry, and gives tables of the trigonometrical ratios, and explanations of the solution of triangles plane and spherical.

The first part gives a general account of the new system, which is explained in a number of distinct propositions. At the very outset we find that Copernik had not emancipated himself completely from the old fallacies: he had no idea of the stars being distributed throughout space; he still imagined them to be fixed on to a celestial sphere outside the sun, moon, and planets; but he could hardly call it the "primum mobile," for he supposed it to be at rest. His first proposition is that the universe is spherical, and the arguments by which he supports this prove that he is not free from mediæval reasoning; they are of the old Aristotelian sort—that the sphere is the most perfect figure and contains the largest amount of space, that the sun and moon and all the heavenly bodies are of this form, and so on.

Next it is proved that the earth and sea together form a globe, and the same reasons are given for this belief as those for which we now hold it. It is then demonstrated that if only the celestial sphere be sufficiently large, there is no reason why the earth should not revolve round the sun as well as rotate on its axis: the reasons for this are the same as those by which the Greeks knew that the celestial sphere must be incomparably larger than the earth. If the earth revolve round the sun, the fact that no two stars appear to move further apart or nearer together in the course of a year proves that they must be at a distance from the observer incomparably greater than the diameter of the earth's orbit. This is followed by reasons for believing that the fixed stars actually are at an enormous distance. The author then passes on to answer the altogether foolish reasons popularly given in his day to prove that the earth must be at the centre of the universe, and gives equally futile reasons of his own to prove that the earth cannot be at the centre.

The next point considered is the natural state of motion of matter; and Copernik's views on this subject are altogether wrong. He conceives that matter is distributed into whole bodies, and that each of these wholes moves naturally in a circle or combination of circles, but that, when any one whole body is separated into parts which are then left free to move, all the parts rush together in straight lines. Hence the earth with all it contains, and each of the planets, revolve in circles or combinations of circles, but any body such as a stone lifted off the earth's surface and then left free will rush in a straight line towards the centre of the earth.

He still holds to the notion that there must be a centre of the world, and places the sun there, with the planets, of which the earth is one, revolving round him. He then shows that the earth may have several different sorts of motion at the same time. Lastly, he proves that the revolution of the earth and planets round the sun would explain the general character of the apparent motions of the planets; and, with a diagram of circles representing their orbits, which has ever since been called the "Copernikan system," he closes the first book.

The second book treats of the doctrine of the sphere, showing what its circles mean according to Copernik's theory, and then, after a description of his instruments, there is a catalogue of the chief fixed stars, with their latitudes, longitudes, and apparent magnitudes.

The third book treats of the equinoxes and solstices, and gives a history of observations proving the precession of the equinoxes. Again we meet with a survival of the old fallacy. Copernik cannot get rid of the notion of crystal spheres carrying the planets; he fancies that the poles of the earth are somehow fixed into such a sphere, which carries it round the sun; hence the axis of the earth would, if produced, always pass through a point in a line through the centre of the sun, perpendicular to the plane of the ecliptic, which we must now regard as the path of the earth about the sun, and not of the sun about the earth. To obviate this, he imagines the earth's axis to be inclined always at the same angle to the plane of the ecliptic, but to have a conical motion in the opposite direction to the earth's revolution, such that each pole moves in a circle parallel to the plane of the ecliptic, and describes an angular revolution of 50″ a year more than 360° about its centre; this would keep the axis of the earth nearly parallel to itself, but not quite, for it would have a conical motion in the opposite direction to the revolution of the earth of 50″ a year. Copernik proves with complete success that this would explain the phenomena of precession.

He next considers the different values observed for the obliquity of the ecliptic. Eratosthenes, about B.C. 270, had found it to be 23° 51′ 13″; Al Batani, in the ninth century, had made it 23° 51′; Walther, at the close of the fifteenth century, had found it to be 23° 29′ 47″. This showed a continual diminution, which could scarcely be accounted for by errors of observation. Copernik shows that the libration or tilting of the earth's axis would produce this effect. In this book the times of the equinoxes and length of the tropical year are fixed, and tables of the prosthapheresis * and nychthemeron are given. Since the sun moves at an irregular rate among the stars in the course of a year, while the earth rotates at a uniform rate, the length of the natural day, or

* *Vide*, p. 22.

period of time from noon to noon, is not the same at all periods of the year ; the nychthemeron is the time that must be added to or subtracted from the time calculated on the assumption that all days are of the same length, in order to find the true time.

The fourth book treats of the movements of the moon, which revolves round the earth, and so is carried round the sun. Tables are given of the lunar prosthapheresis, and an estimation is made of the distances of the sun and moon from the earth, and of their magnitudes, which, though woefully inaccurate is yet nearer the truth than any preceding statement. The book concludes with tables for the prediction of eclipses.

The fifth book treats of the relative distances from the sun, and times of revolution, or "periodic times," of the different planets. It will, perhaps, be convenient to explain how these could be ascertained. Let us first suppose (Fig. 1) the earth

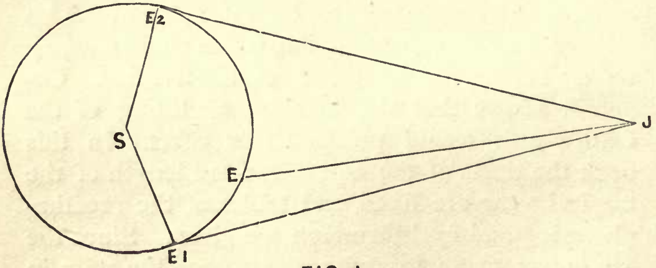

FIG. 1.

E to revolve once a year in a circle about the sun S, and a planet J to remain fixed outside the orbit of E, but in its plane. If E move perfectly smoothly in its orbit, an observer on it will imagine he is at rest, and all he will know about the position of J will be the direction with respect to the stars of the line drawn from his eye to J. This line E J will move from the position E_1 J into that of E_2 J, and back to E_1 J in the course of a year; so that J would appear to an observer on the earth to oscillate backwards and forwards through an angle equal to E_1 J E_2 once in a year. But now (Fig. 2) suppose E to be at rest, and J to move in

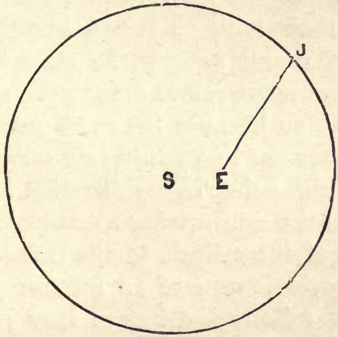

FIG. 2.

a circle about S, once in twelve years; then it will appear to an observer on the earth to move forwards continually, and complete its course round him in twelve years. Now suppose these two movements to go on together. J will then appear to an observer on the earth to oscillate once a year backwards and forwards, owing to the motion of E_1, but its forward sweeps will be much longer than its backward sweeps, owing to its own motion about S, and it will appear to move on the whole once round E while it moves once round S. This is the explanation of the apparent motions of the "superior planets," or those whose orbits lie outside that of the earth. From this we see that the periodic times of these planets can be found by watching how long they take to get once round among the stars.

With regard to the "inferior planets," or those whose orbits lie inside that of the earth, their apparent oscillations in front and behind the sun are due to their motions round him; while they seem to get round among the stars because the sun appears to move round among the stars, and he appears to do so because the earth revolves round him, and hence the line joining an observer on the earth to the sun—that is, the direction in which the observer sees the sun—makes a complete revolution in the plane of the ecliptic in the course of a year. Hence the periodic time of an inferior planet may be found as follows:—Observe how many times it appears to oscillate in a year, add one to this number to account for the revolution of the earth, divide a year by the number thus obtained; the result is the periodic time of the planet.

The loops in the apparent paths of the superior planets are therefore due to the earth's revolution, and their general movement round the celestial sphere to their revolution round the sun. The loops in the apparent paths of the inferior planets are caused by their revolutions round the sun, and their general apparent motion round the celestial sphere by the earth's revolution.

It may be convenient to point out here how the relative distances of the planets from the sun may be roughly found on the Copernikan system.

To find roughly the distance from the sun of an inferior planet relatively to that of the earth, all we have to do is to observe its greatest apparent angular distance either in front of or behind him. At the instant when the planet is at this distance (*vide* Fig. 3), we know that the line joining it to the

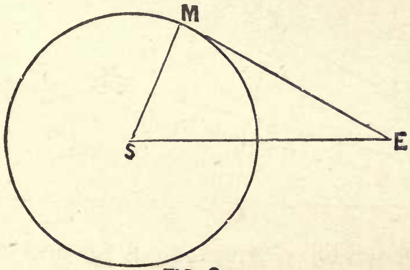

FIG. 3.

earth must be a tangent to its path round the sun; assuming that path to be a circle, we then know that the lines joining the sun to the planet and the planet to the earth must be at right angles to one another, and hence, in the triangle formed by joining the centres of the sun, the planet, and the earth, we know two angles, hence we know the shape of the triangle, and therefore the relative magnitudes of its sides.

The case of a superior planet is rather more difficult. We have to make a further assumption which is not quite true, and hence the result is still less accurate. We must assume the planets to move at a uniform rate. In Fig. 4, let S represent the sun, E the earth, and M the planet. Choose a moment when the sun and the planet are in opposition; then M must be somewhere in S E produced. Wait a month; the earth will now be one-twelfth part of its way round S, therefore it will

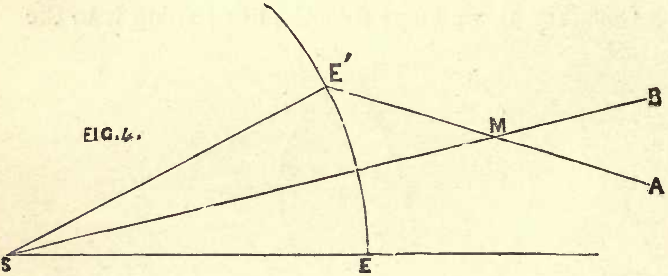

FIG. 4.

be at E′, where S E is equal to S E′, and the angle E′ S E is 30°. Now observe the apparent angular distance of M from S, and make the angle S E′ A equal to this angular distance; then M must lie in E′ A. But by this time the planet will have moved. Suppose its periodic time is two years; then it will be now somewhere in the line S B, where the angle B S E is 15°; hence it must lie at M, where S A and S B intersect. Now, in the triangle M E′ S we know the angles E′ S M and S E′ M, hence we know the shape of the triangle, and therefore the relative lengths of S M and S E′, that is, the relative distances of the planet and of the earth from the sun.

The sixth book treats of the latitudes of the planets, that is, of the inclinations of their orbits to the ecliptic. The planes of the orbits all pass through the centre of the sun, but are not exactly the same; and in order to explain how it is that the apparent progressions and retrogressions of each planet are not in the same straight line, but make flattened loops in the sky, which are sometimes turned on one side, sometimes on the other, of the general direction of their motion—to explain which the ancients had to have recourse to the complicated theory of the librations of the great epicycles—Copernik supposed that the plane of the orbit of each planet was librating, or tilting, in a separate manner.

But when the larger characteristics of the apparent movements had been thus explained by a theory far simpler than the Ptolemaic, their minute irregularities still remained to be accounted for. This Copernik did by assigning to each planet a system of epicycles which was, of course, supposed to move, not round the earth, but round the sun; and it may surprise some of our readers to learn that the greater portion of his book, which is generally supposed to have overthrown the epicyclical theory, is taken up with the determination of the nature, magnitudes, and times of revolution of these epicycles. It is curious to know that his contemporaries thought this the most masterly part of his work; and it must be confessed that Copernik's epicycles explained the apparent movements better than Ptolemy's, and were simpler, in that they were supposed to move at a uniform rate, the equants being completely done away with.

But the retention of epicycles in a mere descriptive theory of the solar system is not necessarily any defect in its truth. To say, as Copernik did, that some system of epicycles must necessarily be true because the planets move naturally in circles is, no doubt, utterly false; but, leaving the causes at work out of consideration, to say that the planets actually do move in a particular combination of circles, which would really describe their actual motions, is as true as to say that they move in ellipses; it is only an unnecessarily complicated way of describing the facts. And more than this, in the most exact astronomical work an epicyclical theory is still used; for, as those of our readers who understand mathematics will see, the expansion of the co-ordinates of a planet in a series of sines and cosines of the time is merely the expression in mathematical language of a system of epicycles.

We have seen, then, that Copernik still left many imperfections in his theory—still left much to be done by his successors. The chief merits of his theory, as compared with that of Ptolemy, were the placing of the sun at the centre of the system of the planets, which made it easier for his successors to determine accurately and completely their movements; the explanation of precession by the conical movement of the earth's axis; and its greater simplicity, in replacing the diurnal rotation of the whole mechanism of the sky by the rotation of the earth, and in doing away with the great epicycle of each planet.

Copernik delayed publishing this work for many years, and those who can conceive of no other motive of human conduct but self-interest, find this inexplicable. For there could be no fear of persecution. This was the period between the last Lateran Council and the Council of Trent; and the Church had far too much to do to trouble herself about the astronomical opinions of her children. Moreover, the friend who most eagerly urged him to publish, and who actually undertook the expense of printing, was a cardinal.

But it is in far other direction than that of self-interest that we must look for Copernik's motives. It was a tremendous thing to attempt the overthrow of a system founded and finished by the most illustrious workers of the past, and buttressed, nay, almost consecrated, by the undoubting belief of centuries. Such an attempt would be certain to rouse the instinctive opposition of all men; absolute clearness of conception and statement, the most searching criticism, the plainest reasoning were needed to disarm the prejudice of the world. And there was no need for eager attack; Copernik could afford to wait, for he knew that truth is very strong.

That no religious scruples impeded his open-minded acceptance of the plain truth is shown in the dedication of his book to Paul III., which contains the only piece of self-assertion we meet with in the story of his life. "If," he there says, "there be some babblers, who, though ignorant of all mathematics, take upon them to judge of these things, and dare to blame and cavil at my work, because of some passage of Scripture which they have wrested to their own purpose, I regard them not, and will not scruple to hold their judgment in contempt."

The printing of the work was, as we have said, entrusted to Andrew Osiander, who seems to have been a contemptible fellow. He took upon himself to write an altogether foolish preface and a vulgar, puffing title-page, of which we will say no more than that it ends with the words, "Igitur eme, lege, fruere." The preface, like the "Declaratio" prefixed to the third book of Le Sueur and Jacquier's edition of the "Principia" of Newton, is a dishonest attempt to reconcile the pursuit of truth with the authority usurped by the Church over the mind of man. It argues that a scientific theory is merely an hypothesis to account for the observed facts, and therefore need not be believed as really true. This is no doubt the case; but it is the duty of an honest man to hold as nearest the truth that hypothesis which most accurately accounts for the facts. The reasoning of this preface, therefore, will excuse no one in believing in the Ptolemaic system as against the Copernikan, which it was its object to do.

Copernik himself, however, never knew of these blots on the first edition of his book, for, while awaiting its publication, now an old man of seventy, he was seized with hemorrhage, which was followed by paralysis. On the 23rd of May, 1543, the first printed copy of his book was sent him by Rheticus. He saw it and touched it. The work that had been given him to do he had finished, and that same night, peacefully as he had lived, he died.

The many imperfections of his system have prompted some to make light of the services Copernik rendered to astronomy. But if we would rightly estimate his work we must try to put ourselves in his position. There has been no space here for any adequate description of the condition of the intellectual world in his day, dominated as it was by the Aristotelians. But if we can realize in any measure the abjectness of the mental slavery in which the world then lay, we shall realize to some extent the mental greatness of the man who first dared to doubt. Without Doubt there can be no true belief, and not only astronomers, but all who in these days value freedom of thought, every man who now follows freely and honestly the leading of the mind and conscience God has given him, owes no small debt to the old monk who, in the solitude of the monastery garden at Frauenburg, thought out the overthrow of the authority of Aristotle.

It is true Copernik cannot be said to have flooded with light the dark places of nature; to have done that is the eternal glory of one stupendous mind alone, whose work we shall consider in the sixth chapter. But as we look back through the long vista of the history of science, the dim titanic figure of the old monk seems to rear itself out of the dull flats around it, pierces with its head the mists that overshadow them, and catches the first gleam of the rising sun—

> "Like some iron peak, by the Creator
> Fired with the red glow of the rushing morn."

CHAPTER III.

ON TYCHO BRAHÉ AND HIS OBSERVATIONS.

AFTER the death of Copernik, his system slowly but surely spread. Erasmus Reinhold (born October 21, 1511, died February 19, 1553), chief Professor of Mathematics at Wittemberg, adopted it, and going through all Copernik's calculations again, corrected them and produced the Prutenic Tables, which were used from 1551, when they were first printed, till the publication of the Rudolphine Tables, by Kepler, in 1627. His determination of the length of the year as 365 days 5 hours 55 minutes 58 seconds, was used in the reformation of the calendar by Pope Gregory XIII., in the year 1582.

The months now used by Christendom are derived from the Romans, and although they, no doubt, had their origin in the changes of the moon, they have long since become entirely arbitrary, and in our calendar there is no attempt to reconcile them with the days and years. The object of the

Gregorian reformation was to reconcile the days and tropical years, and this is done by inserting a day every four years, the year containing the extra day being called leap year. This makes the average length of the year 365¼ days, which is too much; accordingly leap year is omitted every hundredth year. This makes it too short, and so the extra day is only omitted in three out of every four hundred years. So that the rule is: Take the last two figures of the number representing the year; if they are not divisible by 4 without remainder, the year is not leap year; if they are divisible by 4 without remainder, it is leap year, unless the last two figures happen to be two noughts. In this case, if the *first* two figures of the number are divisible by 4, it is leap year; if they are not, it is not leap year. The actual length of the year being 365 days 5 hours 48 minutes 46·05444 seconds, this arrangement will keep the civil year within one day of the tropical for nearly thirty-five centuries.

The most complex parts of Copernik's system were his arrangements of epicycles for the moon and for the planet Mercury. Reinhold materially helped astronomy by pointing out that these would be greatly simplified if the first movement of each of these bodies were in an ellipse, for Mercury round the sun, for the moon round the earth.

Rothman, astronomer to William, Landgrave of Hesse-Cassel, found that Copernik's explanation of precession was unnecessarily complicated, and pointed out that the earth's axis would naturally remain parallel to itself, and that only one slow conical motion was needed to account for the facts.

Mœstlin (born 1550, died December 20, 1631), the instructor of Kepler, explained, for the first time, the faint illumination sometimes visible on the otherwise dark part of the moon. He pointed out that it was due to the sun's light reflected to the moon from the earth.

All these, with Copernik's own pupil Rheticus, firmly held the new system, and were not molested; for in those days men could still profess their belief in what was, after all, only the most probable hypothesis. It was not until the heliocentric system was demonstrated to be true that the Church anathematized and persecuted those who believed in it.

Yet there were some who rejected the new theory or held a modification of it, and it is important to notice that these were not necessarily either prejudiced or stupid. It was not until nearly a century later that the great verifications of the theory were made, and there were great difficulties in the way of accepting the motion of the earth. Apart from the immediate testimony of the senses, it appeared inconsistent with Copernik's own physics; for, according to him, a stone dropped from a high tower ought to move directly towards the centre of the earth, and therefore, if the earth be rotating from west to east, the stone ought to be left behind, and fall to the west of the tower instead of at its foot, as it is observed invariably to do. Copernik himself recognized the difficulty, and supposed that the air carried the stone with it, but this was obviously a very inadequate explanation, and several of the greatest astronomers were hence led to reject the Copernikan system.

One of these, however, suggested a compromise. He saw that if the relative movements of the sun, moon, and planets, and the earth remained as Copernik had left them, the movements apparent to an observer on the earth would be accounted for, no matter what the actual movements were; and he saw also that all the objections urged against the Copernikan system related to that part of it which assigned a movement to the earth. Accordingly he supposed that the earth was at rest, the celestial sphere rotating round it once a day, carrying the sun, moon, and planets with it; but that the latter had independent movements besides, which were exactly those ascribed to them by Copernik; that is, all the planets revolved round the sun. The superior planets, revolving at a greater distance from the sun than the earth's distance, completely surround the earth in their orbits; their motion might therefore be described by stating that they moved in eccentrics, the centres of all of which were the same, and the direction of the eccentricity of which revolved in the plane of the orbits once in a year.

The inventor of this compromise, who seems always to have regarded it as a mere suggestion, and not as certainly the true system, was Tycho Brahé—a worker who rendered services to astronomy scarcely less valuable, though certainly less striking than Copernik himself. It would be impossible to find men less like in character and circumstances than these two. Copernik was of humble origin, and certainly not wealthy; Tycho Brahé was of noble birth, and inherited great riches. Copernik loved solitude and rest, Tycho Brahé restlessly moved from place to place all over Europe; Copernik was calm and peaceful in disposition, Tycho Brahé was a man of violent temper; Copernik was always tender to the prejudices of others, Tycho Brahé delighted in attacking false beliefs. They resembled each other in one point alone—the patient carefulness of their researches, and the strenuous earnestness of their lives.

Tycho Brahé was born on the 14th of December, 1546, at Knudstorp, the ancestral estate of his father, near Helsingborg, in Scania. He was the eldest son and second child of a family of five sons and five daughters. The family was of Swedish extraction, but his father Otto and his grandfather Tycho belonged to a branch settled in Denmark. They were "as noble and ignorant as sixteen undisputed quarterings could make them," and, as is usual in such cases, the art of killing was thought the only occupation worthy of their condescension; and so it was determined that all the five sons should adopt the profession of arms. Tycho, however, and his second brother, Steno, objected to this arrangement; Steno became ultimately Privy Councillor to the King of Denmark. The rest of the brothers were soldiers by profession, but seem to have taken more interest in the political affairs to which their hereditary senatorial rank introduced them; they were, however, none of them particularly distinguished. Of the sisters, only one, the youngest, Sophia, is worthy of note. She was celebrated as a writer of Latin verses, and a good mathematician; she was supposed to be deeply versed in astrology, and took a more reasonable interest in astronomy.

George Brahé, Tycho's uncle, having no children of his own, wished to adopt one of his nephews, and tried to persuade his brother Otto to let him have Tycho while still an infant. This request was not unnaturally refused. But on the birth of Steno, Tycho was stolen away by his uncle, who excused himself to the father by saying that, now that Steno was born, it was but fair that the twofold male progeny should be divided between them. Seeing the affection with which Tycho was treated by his uncle and his aunt Tugera, George Brahé's wife, Otto agreed to the arrangement. And it was well for the future astronomer that he did, for not only did his adopted parents treat him with surpassing kindness, which was continued by his aunt long after her husband's death, but they had a far juster appreciation of the seriousness of life, and gave their nephew what he would never have had at home—a good education.

After having learned to read and write, Tycho began Latin at the age of seven. This, as was the case with every advance he made in learning, was strongly opposed by his family; but George Brahé overcame their scruples by pointing out that it would be well for Tycho to qualify himself for the high offices of State, to which his birth gave him access, by studying law, and for the study of law a knowledge of Latin was indispensable. For five years he worked at Latin, and gained at the same time some knowledge of poetry and literature. During this period, when he was about ten years old, his father died.

In April, 1559, when he was thirteen, he entered the University of Copenhagen, with the view of studying rhetoric and philosophy, as a preparation for the legal profession. Up to this time he does not appear to have inclined to any special branch of learning, or even to have developed any very remarkable powers. But when he had been sixteen months at college, on the 21st of August, 1560, an eclipse of the sun took place. The astronomers had predicted it, and the world, still in the fresh vigour of wakefulness after the slumber of the Middle Ages, watched its arrival with feverish excitement. Tycho shared the prevailing interest, and when he saw the sun darkened on the very day that was predicted, all that strong love of the marvellous, which throughout life was one of the strangest of his many strange peculiarities, impelled him to the study of the science that was able to give its students glimpses into the future of nature. He purchased the "Tabulæ Bergenses" of John Stadius, and began to study the doctrine of the sphere. From this time forth the whole energy of his enthusiasm was thrown into the study of astronomy.

In 1562 he completed his course at the University of Copenhagen, and was sent by his uncle to Leipsic, with a tutor, in order to pursue exclusively the study of law. But his heart was in the heavens, and he only worked at law just enough to save appearances. He spent all his pocket money in astronomical books and such instruments as were within his means. At night, while his tutor slept, he used to watch the stars, with the help of a little celestial globe no bigger than his fist. The "Ephemerides" of Stadius enabled him to find for himself and watch the movements of the planets. Amongst his astronomical books were the Alphonsine and Prutenic Tables, and, comparing their predictions with his own careful observations of the moment of a conjunction of Jupiter and Saturn in 1563, he found that the Alphonsine Tables were as much as a month, and the Prutenic several days, in error. This seems to have determined him to devote his life to their correction.

It sounds strange to our ears, but it is the fact, that Tycho attributed to this conjunction of Jupiter and Saturn the great plague that shortly afterwards devastated Europe. The fresh interest in knowledge then felt, and the giant strides the world was making, not only in the province of science, but in the discovery of new lands, new races of men, new forms of life in the far West, excited delight in all that was wonderful, and stimulated the credulity of men. And so, with the revival of learning, came a revival of the grosser forms of superstition, and Tycho himself was a firm believer in the fallacies of astrology.

If we picture to ourselves the intellectual condition of the world as it then was, we shall perhaps be able to see that in those times of fresh consciousness of the boundless dominion in store for the mind of man, these beliefs which we now know to be superstitious were not so unreasonable as they seem. The chief ground on which we know that the stars can have no relation to the varying fortunes of human life is the fact that predictions founded on that belief are not verified; but in those days there had been no time for a complete verification to be made. The other great objection to astrology is that it is inconsistent with itself. According to its principles, two persons born at the same time in the same place ought to have exactly the same fortunes in life; but this is obviously not the case. And it is satisfactory to know that this consideration led Tycho Brahé, in the end of his life, to reject his early belief in astrology.

At the time of which we are speaking, our astronomer had mastered mathematics without the help of a tutor, and his knowledge was sufficiently advanced for him to commence the correction of the tables; but he found his instruments altogether inadequate. The only means he seems to have had for determining the position of the planets was a sort of pair of compasses, one leg of which he could point at the planet and the other at some known fixed star, and so could measure their angular distance apart. There was at Leipsic, however, an artisan named Scultetus, instrument-maker to Homelius, Professor of Mathematics at the university, and Tycho got him to make a parallactic rule of wood, which seems to have been the first instrument of any accuracy he possessed.

After three years at Leipsic he was on the point of starting for a tour through Germany, when he was recalled to his own country, in the middle of May, 1565, by the death of his uncle, George Brahé, who had made him his heir. Here he met with his first great disappointment. His relations, incapable of any nobler enthusiasm than pride in their aristocratic birth, not only severely rebuked him for neglecting his study of law, but loaded him with ridicule and contempt for following science—a pursuit they regarded as altogether beneath the dignity of a family that for many generations had never stooped to intercourse with the learned. Only one man among them was found to support him. Steno Bille, his mother's brother, henceforward stood to him in much the same sympathetic relation that George Brahé had hitherto done.

Tycho, however, could not stand the contempt and ignorance of his relations, and so next year he left Denmark, and arrived in April at Wittemberg. The plague, however, broke out there, and drove him to Rostock.

Here there happened one of the oddest incidents of his life. Tycho was by no means free yet from the effects of his aristocratic birth and training. He had penetrated the cloud of ignorance that hung over his family, but he was still under the power of their hereditary conceit. He was as yet offensively and foolishly proud, but it was of really admirable attainments. On the 10th of December, 1566, he was present at a marriage feast in Rostock. Here he met a countryman of his own, Maderupius Pasberg, and a quarrel sprang up between them on the comparative excellence of their mathematical attainments. On the 27th they met again at some public games, and the quarrel was renewed; so fierce was it that a duel was arranged, and fought with swords in total darkness at seven o'clock in the evening of the 29th. In the course of the fight Tycho's nose, which from his portraits seems to have been prominent, was cut off. But in a short time he succeeded in constructing a false one, the material of which is variously stated as gold and silver, or brass and putty; most probably it was of some metal enamelled to resemble human flesh, and glued on to his face. We are informed that he constantly carried about with him a box of ointment which had to be applied whenever the nose came off, as it periodically did. To judge from the importance this false nose is given by all his biographers, it would appear that it was the admiration of his age, and exceeded in popular estimation the importance of his astronomical observations.

With the exception of a short journey home, Tycho remained at Rostock until 1569. In that year we find him at Augsburg. He seems to have been greatly struck with the natural and artistic beauty of the town, and, what was more, with the intelligence of the people and the intellectual earnestness of the educated classes. Here he met for the first time with kindred spirits. Chief among these were Paul Hainzel, the burgomaster, and his brother John, a septemvir of the town, both of them greatly interested in astronomy. The former had made and set up in his garden a vast quadrant of fourteen cubits radius, graduated to single minutes of a degree, for taking altitudes, and a sextant of four cubits radius, for measuring the angular distances between the stars, and with these some of Tycho's most careful observations were made. After receiving a visit from Peter Ramers, whom we shall meet again as one of the first martyrs of science, Tycho left Augsburg, and was home again in Denmark in 1571.

This time a very different reception awaited him. His astronomical labours had brought him fame, and he was now known throughout Europe as a leader of thought. His aristocratic relations began, therefore, to think that there might after all be something in the study of science, and so they received him with effusive admiration. He was taken to the court, presented to the king, and generally made much of. His uncle, Steno Bille, however, cherished his nephew's work much more than his nephew's fame, and gave him for an observatory the part of his house best suited for that purpose, and afterwards added the present of another house for the pursuit of alchemy, which the astronomer also wished to work at.

In his labours on this subject we can trace again his inordinate love of the marvellous, though the study itself was, as in the case of astrology, not so absurd in his day as it would be in ours. For many years Tycho pursued alchemy with intense ardour, and he used to speak of it as "terrestrial astronomy," his poetic fancy leading him to regard the precious metals as the stars of the earth.

On the evening of the 11th of November, 1572, as he was returning to supper from his laboratory, he chanced to look up into the sky, and noticed for the first time a new bright star in the constellation of Cassiopeia. This star increased in brightness from day to day, until, in about three months, it was brighter than Jupiter; it then slowly died out, and altogether disappeared in March, 1574. As it altered in brightness it changed colour, and all the facts relating to it were carefully observed and recorded by Tycho. But the chief question which troubled the astronomers of the time was—To what region of space did it belong? Was it an atmospheric phenomenon? or was it a planet? or was it a fixed star? Tycho observed very carefully that it remained at exactly the same place among the stars, and hence he was able to state that it must belong to "the region of the fixed stars."

For, remembering that the earth was believed to be at rest, let C (Fig. 5) represent its centre, and let the smaller circle represent the earth, and the larger the celestial sphere. Then, if O be the position of the observer, and S the new star at

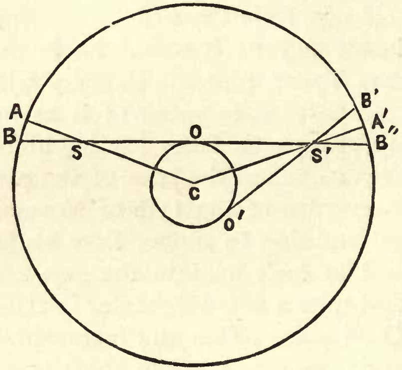

FIG. 5.

sunrise somewhere inside the celestial sphere, he would see it in the direction O S, in a line with some fixed star at B. Now, Tycho observed that the star did not appear to move; therefore it must be carried round at the same rate as the celestial sphere. Hence the line C S A, where A is some star, must always remain straight. When the star S is setting it must be in the position S′, and then

A will be at A′ and B at B′; therefore the observer could not see S in a line with the same star as before, but in a line with some other star B″. But this is observed not to be the case, therefore the new star could not be nearer than the fixed stars.

If the earth were to keep the same position relatively to the celestial sphere, the observer would be at O′ when the new star was at S′; hence we may regard the change of apparent position that would take place if the star were not at a distance practically infinite, as a change of its direction due to the transference of the observer from O′ to O. This change of direction is the angle O′S′O, which is called the parallax of S′, due to the base O O′. If S′ be very distant, this base would be very nearly the diameter of the circle of latitude passing through the observer. Tycho found all the planets to have parallaxes due to this base.

His manuscript on this star was shown to John Pratensis, one of the professors at Copenhagen, and Tycho was asked to give a course of lectures on astronomy at the university. This he refused, but, on the king adding his request, he felt himself obliged to obey. Tycho was still in the trammels of his aristocratic prejudices, for, as he himself afterwards confessed, the reason of his aversion to lecturing was the notion that it was beneath the dignity of one of noble birth. But his mind was very soon cleared of all such notions for in the year 1573 he married a peasant girl of great beauty from his native village of Knudstorp. His relations, who had been sufficiently shocked at his stooping to lecture, and still more at the publication of his tract on the new star, with his name appended, would now have nothing to do with him. The king, however, interfered to patch up their differences, and in time they were reconciled.

Tycho's marriage was an uninterrupted happiness to him, and from this time forth he was free from all social prejudices. Feeling the wrong of the notions from which he had escaped, the whole innate fierceness of his character was called out against all great folk; but with this there grew up in him a wonderful tenderness for those who were poor or in distress, and, in the midst of his careful and laborious life of astronomical observations, he found time to minister with his help to all who could not help themselves.

But the life of a Danish noble involved interruptions to a life of science, and Tycho determined to seek in Germany more congenial society. Leaving his wife and his newly born daughter, Magdalen, at home, he went forth, in 1575, to seek a place of abode. He first visited William, Landgrave of Hesse-Cassel, an astronomer of some learning, who had made a valuable series of observations; but the death of one of this prince's daughters cut short his visit, and he set out on his travels again. He visited Frankfort, passed through Switzerland to Venice, thence returned to Ratisbon, where he was present at the coronation of the Emperor Rudolph II. After seeing Saalfeld and Wittemberg, he finally chose Basle as the place to set up his observatory.

But in the mean time great things were preparing for him at home. William of Hesse had represented to Frederic II., King of Denmark, that the work of such a man as Tycho Brahé would reflect honour on his country, and that it was the duty of the king to aid his plans. Accordingly, Frederic offered to grant the astronomer the island of Huen, about fourteen miles from Copenhagen, as the site of an observatory. The offer was eagerly accepted. £400 a year was settled on Tycho for his life, besides a fief in Norway and a canonry at Roeskilde. £20,000 was given him to build a residence and observatory on the island, and with another £20,000 added by Tycho himself, the famous Uraniburg, or city of the heavens, was begun.

It was situated on a hill in the centre of the island, and was surrounded by a square wall, each side being three hundred feet long. The four angles pointed to the four cardinal points, and at the middle of each side the wall became a semicircle ninety feet in diameter. At the north and south corners were turrets, one for a printing-house, the other for a residence for the servants. Inside was another wall, and between the two a shrubbery, containing about three hundred different sorts of trees. Inside this again was a garden surrounding the principal building, which consisted of two towers for observatories, and the dwelling-house, with a crypt for a laboratory. Statues and pictures of astronomers adorned the building. In the observatories were twenty-eight instruments, some of them graduated to 10″. The foundation stone of this castle of the heavens was laid at sunrise on the 13th of August, 1576, in the presence of Frederic and a company of the chief men in the kingdom, by Charles Danzeus, a man of great learning, a friend of Tycho Brahé, and French Ambassador at the court of Denmark.

For the next twenty years Tycho worked in this splendid temple of science. In 1577 he began his observations. On the 13th of November he discovered the comet which was the subject of his second work. By the same method as that used for the new star of 1572, this comet was found to have a small parallax, and, by the changes in its value, it was proved that the comet's path crossed the position of the crystal spheres, which the ancients, and possibly even Copernik himself, imagined to carry the planets. This was the first demonstration of the fallacy of the notion of material spheres, which was still held by the Aristotelians. In 1582 Tycho began a systematic series of observations on the planets, especially Mars, in order to correct the existing tables.

As time went on, he gathered round him at Huen a school of young astronomers, whom he educated in the use of astronomical instruments and who helped him in his observations. Many of these were chosen from among the lowest of the people, but they were treated with the same consideration as the greatest ones who visited him. He was now the foremost man in the intellectual life of Christendom, and most of the princes of Europe paid visits to Uraniburg. They were always received with great magnificence, but some of them were shocked that the peasant girl who had become Tycho's wife was not kept in the background, as they desired, but was put forward by her husband to entertain the royal guests. But all who entered Uraniburg had to leave behind their social prejudices; and Tycho took a rather malicious delight in exposing and ridiculing the mistakes of royal pretenders to knowledge. It is said that he even took the trouble to make a number of foolish toys—mice running round in cages, models of the sky that moved by clockwork, little windmills, and so on; and to some of the more ignorant of his great guests he showed these as his instruments, and they went away perfectly happy. He could only trust himself to reveal his true instruments to serious, hard-working men, who would look upon them as something more than a nine days' wonder.

His love of the marvellous was curiously illustrated by his keeping a maniac in his house. This man, whose name was Lep, was supposed to have the power of predicting future events. At the great banquets at Uraniburg, Lep was always present, and whenever he spoke, all had to keep silence and listen.

To the rich and the noble Tycho appeared a red-haired, fiery, rude, contradictory little man, with a false nose and a violent temper, proud and insolent to the last degree. But there was another side to the picture, known only to his own family and the poor of Huen. Day after day this fierce, proud man would watch by the side of some sick peasant, devoting all the medical knowledge he had gained in his study of alchemy, and all the patience of the most laborious observer the world has ever seen, to the endeavour to heal those whose lives in that day were little accounted of.

This life Tycho Brahé led for twenty years, and then a change passed over his fortunes. In 1588 his friend Frederic died, and Christian IV., a mere boy, succeeded to the throne. Tycho had stirred up enemies among the privileged classes. The clergy hated him for the independence of his religious convictions; the doctors hated him for administering medical help to the poor gratis; the courtiers hated him because all the great folk of Europe passed them by to pay homage at Huen; the chancellor, Christopher Walchendorf, hated him for private reasons. On one occasion, while staying at Huen, this man had brutally kicked one of the deer-hounds which James, afterwards King ot England, had given Tycho while on a visit to Uraniburg. Tycho, who hated cruelty with all the fierceness of his nature, gave Walchendorf a piece of his mind, in much the same terms as he would have used to one of his serfs for a similar offence. Ever after the chancellor waited an opportunity to ruin him. Perhaps also he was influenced by the knowledge that the Canonry of Roeskilde, which Tycho held, had formerly been part of the emoluments of his own office. The young king, on a visit to Uraniburg, had himself been rudely exposed, when pretending to a knowledge of mathematics he did not possess. The foreign princes were not fond of a man whose democratic notions took so practical a turn. The Duke of Brunswick once admired some working model at Uraniburg. Tycho gave it him, under the promise that a cast of it should be sent him, in order that another might be made. This promise the duke neglected, and paid no attention to Tycho's remonstrances. In a description of Uraniburg, which Tycho wrote soon after, he was careful to publish this anecdote.

From all these facts, Tycho suspected that evil times were in store for him. In a letter to the Landgrave of Hesse, written in 1591, he darkly hints at the possibility of having to leave Huen, but comforts himself with the thought that every land is the home of a great man, and that whithersoever he went the blue sky would still be over his head. In 1592 his friend the landgrave died, and he was at the mercy of his enemies. His pension, his estate in Norway, his canonry at Roeskilde, were successively taken away, and in 1597 want of means obliged him to remove to a small house in Copenhagen. Not content with ruining his worldly fortunes, his interested enemies attempted to disgrace him. A commission was appointed, all of whom were totally ignorant of astronomy, to inquire into his labours. They reported that his work was not only useless, but noxious. At the instigation of the malicious Walchendorf, an attack was made upon Tycho in the streets, and one of his servants was injured in the affray.

Tycho resolved to quit his ungrateful country. With his wife, his five sons and four daughters, and his devoted disciples, among whom were Tengnagel who married his eldest daughter, and Longomontanus, afterwards an astronomer of note, he crossed over to Germany about the end of 1597. After receiving the generous hospitality of Count Henry Rantzau at the Castle of Wandesberg, near Hamburg, he was invited by the Emperor, Rudolph II., to take up his abode in Bohemia. In the spring of 1599 he accepted this invitation, and settled at Prague.

He was received with the utmost kindness. A splendid house was assigned to him in the city; a pension of three thousand crowns was settled on him for life, an estate was bestowed on his family, and a choice of three castles was given him for an observatory. He chose that at Banach, about twenty miles north of Prague, and set up his instruments there on the 20th of August. Here he lived for two years more, leading the same strange, weird, earnest life he had led at his beloved Uraniburg, gathering round him a new school of young philosophers, whose labours he directed, and whose actual wants he in some cases relieved.

But though every kindness was shown him, he was still a stranger in a foreign land. The ingratitude of his country weighed upon his mind, and he became morbidly despondent. In this mental depression his early superstitions seem to have returned upon him; and even in the moments when the clouds lifted from his spirit, he turned his thoughts and conversation to his latter end. On the 13th of October, 1601, he was seized with a painful disease, and on the 24th he died. His last illness was typical of his fierce life. In his delirium he was constantly crying, "Ne frustra vixisse videar!" "Oh that I may not be found to have lived in vain!" His life had been given him, not to enjoy, but to labour, and in the yearning of this, his life's idea, he passed away.

Tycho's greatest discoveries had reference to the motion of the moon. He rediscovered the inequality called the variation, first noticed by Aboul Wefa; the nature of which is that the moon is always in advance of her place, as calculated by Ptolemy or Copernik, from syzygy to quadrature, and behind it from quadrature to syzygy; the maximum of this variation taking place in the octants, that is, the points equally distant from syzygy and quadrature, its value there being about 40′. He found also another inequality, usually called the "annual equation," by which the moon is behind her computed place while the sun moves from perigee to apogee, and before it in the other half of the year. And he discovered that, though the moon's nodes on the whole regress, they do not do so constantly, but that they sometimes stop and advance for a short distance, her inclination increasing or diminishing through an angle of 20′ according to the position of the nodes.

From a man of his fierce character we might have expected brilliance of intellect, flashes of insight into nature, ingenious suggestions of new systems; but this is just what we do not get. Tycho Brahé added nothing to the theory of science, but the violent earnest man laid deep the foundations in minute observation of the whole majestic structure of modern astronomy. He has left us a catalogue of the exact positions of a thousand of the fixed stars, still used to determine their slow proper motions. He has left us a multitude of observations of the exact positions of the planets night after night for twenty years. And while Ptolemy can never be depended upon to within half a degree of error, Tycho Brahé, with the same sort of instruments, never made an error of a single minute in any one of his thousands of observations, never stated the position of any of the heavenly bodies wrong to the extent of the thirtieth part of the full moon's apparent diameter.

Whence came it that the wealthy, high-born, passionate man excelled not in brilliant insight but in patient care? Whence came it that, in the moment of death, his mind was brooding, not over the bitter ingratitude of his country, but over the inadequacy of his own marvellous labours? Perhaps we may find some clue to the mystery in that allegorical device he caused to be engraved on one of his brazen quadrants at Uraniburg.* A young philosopher is seated on a stone, holding in one hand a celestial globe, and in the other a book. Beside him is a tree in full foliage on one side, while on the other its branches are withered and leafless. Upon a table close by are displayed all things that the world values—gold and jewels, sceptres and crowns; but the figure of Death is surrounding them all with his arms. Over the picture on one side is the inscription—

"Vivimus ingenio, cætera mortis erunt;"

and on the other side—

"Vivimus in Christo, cætera mortis erunt."

Within the green foliage of the tree are hung hieroglyphical references to the life and teaching of our Saviour, the meaning of which, Tycho said, was to show "that no man can be made happy and enjoy immortal life but through the merits of Christ the Redeemer and the Son of God; and by the study of His doctrines and the imitation of His example."

* The description of this device is taken from Brewster's "Martyrs of Science."

CHAPTER IV.

ON KEPLER AND HIS LAWS.

FOR a few hours before Tycho Brahé's death, that fierce delirium, in which the secret purpose of his life was laid bare, left him. In this short period of peace before his end, he gathered his household around him, and, expressing an earnest desire that his own labours might redound to the glory of God, he ordered his family to be careful that these labours should not be lost, he exhorted his disciples to persevere in the work they had begun under his direction, and, in particular, he entrusted one of them, John Kepler, with the duty of completing and editing his corrected tables of the planets' movements; and he ordered them to be published under the name of the "Rudolphine Tables," in honour of his benefactor, the emperor.

The man thus singled out for this great honour was worthy of it. John Kepler was not only, if we except Newton, the Master, as great a genius as appears in the annals of science, but he was one of the purest and noblest souls of whom history speaks. Working now upon the foundation of Tycho's observations, he applied himself to the task of correcting and to some extent recreating the theory of astronomy. His labours, therefore, were confined to the two last steps of the complete induction—theorizing and verification; and for each of these he was marvellously well fitted.

He was gifted with the most luxuriant imagination, which enabled him to grasp and link together by his theories the most distant points of his knowledge. Indeed, so wild was this faculty in him, that many have regarded his wonderful success as an accident. Even Laplace has said, "It is disappointing for the human mind to see this great man plume himself with complacency on his chimerical speculations, and regard them as the life and soul of astronomy." * Following Dr. Whewell, we venture utterly to disagree with this criticism. And we think that the very work of Kepler himself proves that the wilder his imagination the more likely is the student of nature to discover truth, provided only he be scrupulously careful and honest in verifying his theories. In this Kepler was laboriously careful. He cared for nought but truth, and never cast so much as a regretful look at theories over which he had spent years of toil, when once they were proved to be false.

* Quoted in Whewell's "History of the Inductive Sciences," i. 432; from Laplace's "Précis de l'Hist. d'Ast.," p. 94.

His works give a most striking view of the process by which great discoveries are made; for, careless of his own fame, and desiring only to extend the bounds of human knowledge, he has done one thing that probably no one else ever dreamed of. While most discoverers would desire to conceal the follies they may have fallen into in their investigations, Kepler has written a complete account of all the mistakes he ever made. "For it is my opinion," said he, "that the occasions by which men have acquired a knowledge of celestial phenomena are not less admirable than the discoveries themselves;" and again, "If Christopher Columbus, if Magellan, if the Portuguese when they narrate their wanderings, are not only excused, but if we do not wish these passages omitted, and should lose much pleasure if they were, let no one blame me for doing the same."

At the outset of his astronomical studies, Kepler was led by his instructor Mœstlin to believe in the Copernikan system; and it must be remembered while reading the following pages that throughout his life Galileo was making those verifications of this system, which we shall consider in the next chapter, and which must lead every fair-minded man to believe in its general truth. The theory of astronomy, therefore, started in Kepler's hands where Copernik had left it. But the work of Kepler introduces us to a new aspect of the science.

There are three questions which may be asked about the heavenly bodies. First, *What* are their actual positions and movements so as to produce the complicated positions and movements we observe? Secondly, *Why* are their positions and movements what they are? And thirdly, What *are* the heavenly bodies themselves? The answer to each of these questions is the subject of a separate branch of astronomy. The answer to the first is called "Formal Astronomy;" the answer to the second, "Physical Astronomy;" but as nearly everything that is known about the third has been discovered within the last thirty years, no very generally accepted name is given to the subject that treats of it. For want of a better word, we shall speak of it as "Chemical Astronomy;" and there can be no doubt that it is this direction that the future development of the science will take.

Now, it is obvious that the old epicyclical theory was both a formal and a physical theory; it both stated how the planets moved, and gave a mechanism to produce their movements. Copernik also gave a physical explanation of the motions as they were according to his theory; but we have already seen that this explanation was wrong. The effort of Kepler, therefore, was devoted mainly to physical astronomy. Although he incidentally corrected the formal theory, it was the *causes* of the facts that he sought. "There were three things in particular," he writes, "of which I pertinaciously sought the causes why they are not other than they are: the number, the size, and the motion of the orbits."

In the youth of Kepler it would have been impossible to detect any promise of the future astronomer. He was the eldest son of his parents, and was born at the imperial city of Weil, on the 21st of December, 1571. Being a seven-months' child he suffered from the most delicate health all his life, but particularly in his childhood. His father, Henry Kepler, was son of the Burgomaster of Weil, but had squandered most of his wealth, and was serving in the army of the Duke of Wirtemberg as a petty officer when his son John was born. The mother, Catharine, whose maiden name was Guldenmann, could neither read nor write, and was a fierce virago, who ill treated her eldest son and petted his two worthless brothers.

At five years old John Kepler was sent to school at Leonberg, and his mother followed her husband to the Netherlands, where he was fighting under the Duke of Alba. On their return they found that their son had been at the point of death from the small-pox, and that a friend for whom Henry Kepler had become surety was bankrupt. The Keplers had now lost the remains of their fortune, and were forced to sell the little property they had left, and keep a tavern at Elmendingen. John was taken from school to act as servant. So little does the world care for its great ones that it set the born "legislator of the heavens" to be pot-boy in a public-house.

In 1585 the father and mother both fell ill with the small-pox, and John was again at the point of death from some violent illness. On the 26th of November, 1586, he was fortunate enough to be admitted to the school at Maulbronn, established on the suppression of the monastery there at the Reformation, and maintained at the expense of the Duke of Wirtemburg. This school was preparatory to the University of Tübingen, and the scholars, after a year at the upper classes, were examined at the university for the degree of Bachelor, and then returned to the school with the title of "veterans." After completing their studies and taking up residence at Tübingen, they were admitted to the degree of Master. Kepler's studies in pursuing this course were interrupted by successive illnesses and quarrels in his family. His father, finding the tavern at Elmendingen a failure, enlisted in the Austrian army, then operating against the Turks, and is no more heard of. His mother, having been treated, says Hantsch, his earliest biographer, "with a degree of barbarity by her husband and brother-in-law that was hardly exceeded even by her own perverseness," quarrelled with all her own relations. In the midst of this confusion, Kepler took the degree of Bachelor on the 15th of September, 1588, and that of Master in August, 1591, securing the second place in the examination.

It was while attending the lectures of Mœstlin, Professor of Mathematics at the university, that he was instructed in the Copernikan system. He adopted it, and defended it in the disputations of the students, and even wrote an essay on the primary motion. Still so far he had no special liking for astronomy; his interest seems to have lain rather in the direction of metaphysics and theology.

It has been remarked that "if Cleopatra's nose had been half an inch shorter, the history of the world would have been changed;" and although this "Cleopatra's nose" theory is in general untrue, it seems to have held good in the history of science about the year 1594, for in that year the astronomical lectureship at Grätz, the chief town of Styria, fell vacant by the death of George Stadt, and was offered to Kepler, who was looking about, penniless, for something to do. He was reluctantly forced to accept it, as he says, "with many protestations that I was not abandoning my claim to be provided for in some other more brilliant profession."

He, however, set to work earnestly to master the science; "And as in Virgil," he says, "'Fama mobilitate viget, viresque acquiret eundo,' so it was with me that the diligent thought on these things was the occasion of still further thinking; until at last, in the year 1595, when I had some intermission of my lectures allowed me, I brooded with the whole energy of my mind on this subject."

After trying a number of plans to account for the "causes" of the number, size, and motion of the orbits of the planets, every one of which cost him much labour, and every one of which he honestly rejected when he found it was inconsistent with the facts, he at last hit upon this : "(The orbit of) the earth is the circle, the measurer of all. Round (the sphere of which it is a great circle) describe a dodecahedron ; the (great) circle (of the sphere) including this will be (the orbit of) Mars. Round this (sphere) describe a tetrahedron ; the (great) circle (of the sphere) including this will be (the orbit of) Jupiter. Describe a cube round this ; the circle (of the sphere) including this will be (the orbit of) Saturn. Then inscribe in (the sphere belonging to) the earth an icosahedron ; the circle (of the sphere) inscribed in it will be (the orbit of) Venus. Inscribe an octohedron in (the sphere belonging to) Venus ; the circle (of the sphere) inscribed in it will be (the orbit of) Mercury." * In this way Kepler deduced, from the fact that there are but five regular solids, the fact, as was then believed, that there are only six planets. The relative sizes of the orbits agreed so nearly with the facts as then ascertained, that he rashly attributed the small divergence to errors of observation. Of this, his first discovery, though only an imaginary one, he writes, "The intense pleasure I received from this discovery can never be told in words. I regretted no more the time wasted ; I tired of no labour ; I shunned no toil of reckoning. Days and nights I spent in calculations, until I could see whether this opinion would agree with the orbits of Copernik, or whether my joy was to vanish into air." The book in which this theory was published, with a complete account of all the

* The words in brackets are inserted to make the meaning clear.

rejected theories, came into the hands of Galileo and Tycho Brahé. They both praised the ingenuity and honesty of the author, but Tycho wisely advised him "to lay a solid foundation for his views by actual observation, and then, by ascending from these, to strive to reach the causes of things."

One beautiful trait of Kepler's character appears in connection with this work. One of his rejected theories assumed a new planet between Mars and Jupiter. Kepler was afraid this might be mistaken by a careless reader to be an anticipation of Galileo's discovery of the satellites of Jupiter; and so in a subsequent edition of this work, published in 1621, he adds a note referring to his supposed planet, "Not circulating round Jupiter, like the Medicæan stars. Be not deceived. I never had them in my thoughts."

In this same work he suggests vaguely that the cause of the motion of the planets might be something in the nature of spokes issuing from the sun, rotating with him, and pushing the planets round in their orbits; but this idea he failed to verify.

In 1597, after some delay caused by her family, Kepler married Barbara Müller von Muleckh, a widow for the second time, though she was only twenty-three years old. Her fortune was much less than her relations had led him to expect, and the income from his lectureship was exceedingly small, and so he found himself in great poverty. To add to these troubles, the religious wars were devastating Germany. Kepler was himself a Protestant, the people of Grätz were in the main Catholics. Finding his life in danger from them, he withdrew into Hungary, and thence wrote several small astronomical tracts to his friend, Zehentmaier, of Tübingen, all of which have been lost.

In 1599 he was recalled to Grätz by the States of Styria, but poverty and personal danger rendered his life wretched; and hearing next year from Tycho Brahé that he had arranged a series of observations on the planets, by which the eccentricities of their orbits could be more accurately determined, Kepler visited him at Prague, and spent three months at Banach. Here plans were concerted by which Kepler might become Tycho's assistant, and keep his lectureship at Grätz, but the religious excitement increasing there, he resigned his appointment. After several attempts to find peaceful employment, Tycho, hearing of his difficulties, invited him with his wife to Prague, promised to provide for him himself in the mean time, and to endeavour to get appointment for him from the emperor.

Kepler and his wife then set out for Prague; but falling ill of ague on the way, they were detained for several months, and the last farthing of their little store of money was spent. They were forced to apply to Tycho for assistance, and he gave it freely and with no offensive condescension. For some time they lived on his bounty. During this while Kepler, penniless and ill, seems to have brooded over his ill fortune, until his sense of dependence became unbearable, and, being absent from Prague for a short time, he wrote to his benefactor a violent letter, filled with undeserved reproaches. But he did not yet know Tycho Brahé; and it is touching to see how tenderly the great, fierce man, whose scorn so outraged the great folk who came across him, treated poor Kepler. Not a word of rebuke; he calmly and gently reasoned with him, and showed him how undeserved were his reproaches. Kepler was deeply moved when he realized his gross ingratitude, and in a letter in reply he says, "Most noble Tycho, how shall I enumerate or rightly estimate your benefits conferred upon me? For two months you have liberally and gratuitously maintained me and my whole family; you have provided for all my wishes; you have done me every possible kindness; you have communicated to me everything you hold most dear. . . . This being so, . . . I cannot reflect without consternation that I should have been so given up by God to my own intemperance as to shut my eyes to all these benefits; that instead of modest and respectful gratitude, I should indulge for three weeks in continual moroseness towards all your family, in headlong passion, and the utmost insolence towards yourself."

After their reconciliation, Kepler returned to Prague, and in 1601 was presented to the emperor, who made him Imperial Mathematician, on condition that he should assist Tycho. Accordingly, it was arranged that he and Longomontanus should edit and reduce to order Tycho's observations, the latter taking in charge those on the moon and the fixed stars; Kepler, those on the planets. These plans were cut short, first by Longomontanus being appointed to a professorship in Denmark, and then by Tycho Brahé's death.

On the 29th of September, 1604, a new star was observed at the foot of the constellation of Serpentarius. It died out in a few months, and resembled in all particulars that noticed by Tycho Brahé in 1572.

The essential secret of Kepler's untiring labours was the inexplicable intuition of his genius that there must be some subtle relation underlying the apparently most disconnected facts of astronomy, and that if he could only discover this relation, he would therein possess the key that would unlock the mystery of physical astronomy. But he carried this thought too far, and was thereby led into the notion that there must exist some spiritual connection between the movements of the heavenly bodies and the movements of human life. This belief is dangerously near to astrology; but it is important to notice that he completely saw the fallacy of the common notions of the power of predicting events from the stars.

In a passage in his book on the new star, which may be quoted both to show this and also as a good example of the style of his astronomical writings, referring to the star of 1572, and to the fact that that of 1604 appeared when Mars, Jupiter, and Saturn were in the constellations of Aries, Leo, and Sagittarius, the three "fiery" signs—an event which only happens once in eight hundred years—he says, "Yonder one chose for its appearance a time no way remarkable, and came into the world quite unexpectedly, like an enemy storming a town, and breaking into the market-place before the citizens are aware of his approach; but ours has come exactly in the year of which astrologers have written so much about the fiery trigon that happens in it. . . . What it may portend it is hard to determine, and this much only is certain, that it comes to tell mankind either nothing at all or high and weighty news quite beyond human sense and understanding. It will have an important influence on political and social relations; not indeed by its own nature, but as it were accidentally, through the disposition of mankind. First, it portends to the booksellers great disturbances and tolerable gains; for almost every Theologicus, Philosophicus, Medicus, and Mathematicus, or whoever else, having no laborious occupation entrusted to him, seeks his pleasure in studies, will make particular remarks upon it, and will wish to bring these remarks to the light. Just so will others, learned and unlearned, wish to know its meaning, and they will buy the authors who profess to tell them. I mention these things merely by way of example, because, although thus much can be easily predicted without great skill, yet may it happen just as easily, and in the same manner, that the vulgar, or whoever else is of easy faith, or it may be crazy, may wish to exalt himself into a great prophet; or it may even happen that some powerful lord, who has good foundation and beginning of great dignities, will be cheered on by this phenomenon to venture on some new scheme, just as if God had set up this star in the darkness merely to enlighten them."

Kepler, on the death of Tycho Brahé, was appointed to his position, and he now hoped that his pecuniary difficulties were at an end. The greatness of his post and its nominal salary excited the envy of the courtiers; but their ill will was soon assuaged, for the salary was never paid him; and he continued for many years, as he said, "begging his bread from the emperor" at Prague. He could not obtain food enough for his family, and the publication of so expensive a work as the Rudolphine Tables was out of the question. During these years he published several smaller works, among which were a treatise on the motions of comets, in which he suggests that their apparent movements might be explained by supposing that they moved in straight lines; and the "Supplement to Vitellion," an optical work of great value, but only incidentally connected with astronomy.

We must now pass on to consider the labours which resulted in the publication, in 1609, of his greatest work, and one of the greatest works in the history of science, the "Commentaries on the Motions of Mars."

When Kepler joined Tycho Brahé at Prague, he found him engaged in constructing from his observations tables of the motions of Mars. We have already pointed out the main characteristics of Tycho's system, but one detail must be mentioned —he reintroduced the notion of an equant, which Copernik had rejected. Kepler adopted the heliocentric hypothesis, but also supposed the planets to move at a uniform rate round an equant.

We have already pointed out that the relative positions and motions of the bodies of the solar system are independent of their actual positions and movements. Supposing, then, the sun to be at rest, Tycho's views on the position of the equant were equivalent to the following assumption:— Let S (Fig. 6) represent the sun, C the centre of

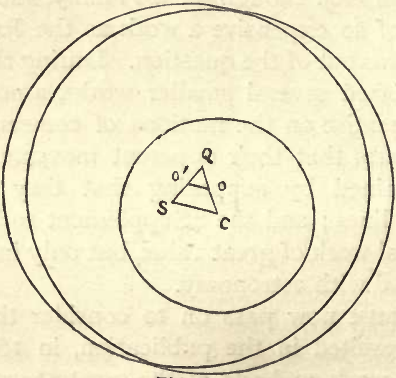

FIG. 6.

the earth's orbit, Q the position of the equant of the planet's orbit; then the centre of the planet's orbit is at O, which bisects Q C. This did not account for the facts sufficiently accurately, and Kepler's first suggestion was that the centre of the planet's orbit lay, not at O, but at O′, which bisects Q S.

In attempting to verify this theory, Kepler made two discoveries. First, that the line of nodes, the line along which the planes of the earth's orbit and that of the planet intersect, passes, not through C as he had previously supposed, but through S. This was afterwards found to be true for the line of nodes of the orbits of any two planets. And secondly, he found that the planes of the orbits of the planets were not librating, but were fixed, and that the loops of their apparent paths were caused by the fact that the earth carries the observer sometimes to one side of the orbit of each of them, sometimes to the other side of it.

Finding, however, that the "bisection of the eccentricity" would not account for the observed positions of the planet, he tried the effect of supposing that the centre of its orbit divided the line S Q in some other ratio than that of equality. When he had made any supposition, he then had to go through laborious numerical calculations to find where the planet would appear to be at any instant, in order to compare it with Tycho's observations. These calculations involved the most wearisome drudgery, for in those days logarithms were unknown. Kepler went through the whole of this process of calculation no less than seventy times before he arrived at a ratio that agreed at all with the facts. At last he hit upon one that for several successive computations only gave an error of two minutes, but to his inexpressible disappointment he found, on further deductions from it, that the computed places of the planet sometimes differed as much as eight minutes from Tycho's observations.

Here an ordinary man would have been sorely tempted. Tycho might have made so small an error in his observations. It was a cruel thing to sacrifice the result of four years of toil—toil that had produced a theory so nearly true. Not so thought Kepler; he had no desire for aught but truth; and he had known Tycho Brahé; and he knew that Tycho Brahé was never eight minutes wrong in any observation. And so he bravely set to work to go over the whole weary way again, declaring that "upon this eight minutes he would yet build up the true theory of the universe."

So far he had assumed that the earth moved in a circle at a uniform rate; after strenuous thought and laborious calculations, he found that the error could be reduced by supposing an equant for the earth's motion, situated according to the principle of the "bisection of the eccentricity."

Still a considerable error remained, and Kepler attempted to simplify the idea of motion round an equant. We have already pointed out that he had suggested a theory of spokes from the sun, pushing the planets round. Observing that those planets moved slower which were further from the sun, he supposed these rays of solar influence became in some uncertain manner weaker as they got further from the sun. Now, according to this theory of the "bisection of the eccentricity," the earth would move from A to B (Fig. 7) in its orbit in the same time as it would move from *a* to *b*; that is, it would move quicker the nearer it is to S, the sun. Having his theory of spokes in his mind, it then occurred to him to try to do away with the equant and connect the rate of motion of the earth directly with its distance from S.

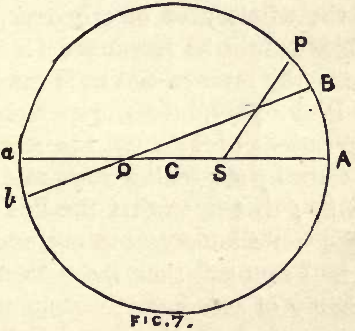

FIG. 7.

His first notion was that it was inversely proportional to the distance. In order to verify this theory, he divided the orbit into 360 equal parts, and calculated the distance of S to each of these divisions. This calculation was in itself an enormous undertaking in the elementary condition in which mathematics then was. He could find by observation the time taken by the earth to move from A up to some division at P, and then, if his theory were true, the sum of all the distances between S A and S P ought to be to the sum of the whole 360 distances as the time taken by the earth to move from A to P is to a year.

This, however, was not an exact verification, because it assumed that the earth moved at a uniform rate between every two consecutive divisions; and moreover, the time required for so laborious a calculation was enormous. Accordingly it occurred to Kepler to compare the area A S P, bounded by A S and P S and the curved path A P, with the whole area of the orbit, and to his inexpressible delight he found that, if he supposed them to be in the ratio of the time taken to move from A to P to a year, his theory was brought near to the observations of Tycho. He still further reduced the error by supposing Mars also to move in the same way; that is, so that the line joining the planet to the sun should sweep out equal areas in equal times. From this time the equant disappears from the theory of astronomy.

Still, however, a small error was left. It was still the fashion to represent small irregularities by epicycles, and Kepler saw that if the centre of an epicycle moved in an eccentric round the sun, sweeping out equal areas in equal times, and the bar that carried the planet revolved at a uniform rate about this moving centre, the actual path of the planet would lie wholly within the eccentric circle, with which it coincides at aphelion and perihelion. There is no space here to follow his laborious and untiring labours to discover what actual path would account for the facts; it must suffice to state that ultimately he arrived at the idea that this path is an ellipse, with the sun in one of the foci.

These two theories—that the planets move in ellipses, with the sun in a focus of each, and that they move in these orbits in such a manner that the line joining each to the sun sweeps out equal areas in equal times, are called respectively Kepler's first and second laws. To his inexpressible delight, he found that together they would account for the motions of the planets to within the possible error of Tycho Brahé's observations; and from this time forward we hear no more of epicycles.

In presenting this work recounting the story of his triumph over Mars to the Emperor Rudolph, Kepler begged for the sinews of war to carry on campaigns against the rest of the planet's family—father Jupiter, brother Mercury, and so on. An order was accordingly issued for the payment of the arrears of his salary; but it was never executed, and Kepler was in great distress.

In the next year, 1610, he suffered every possible affliction—extreme poverty; his wife, ill with despondency, was taken with fever and epilepsy; his favourite son died of the small-pox; Prague was invaded, and the army quartered on the citizens; and, to complete their misfortunes, the Austrian troops introduced the plague into the city. Kepler went into Austria to try to obtain a professorship at Linz; but the emperor would not let him leave Prague, and renewed his promises of payment, which were as usual broken. On his return Kepler found his wife in a decline; almost immediately she caught a fever, and died in eleven days.

His marriage had not been happy, but the cares of his motherless children so interrupted his work that he soon determined to marry again. His own account of the negotiations that preceded his second marriage are most quaint. Some novelists are fond of depicting the phenomena of conflicting passions in a single breast, but the wildest writer of fiction never ventured to suggest such an *embarras de richesse* as Kepler suffered from; he hesitated among no less than eleven ladies. The first, however, that he proposed to refused him; most of the rest he came to the conclusion, for one reason and another, to give up all thought of, but, as he himself naïvely remarks, "The mischief in all these attachments was that, whilst I was delaying, comparing, and balancing conflicting reasons, every day saw me inflamed with a new passion."

After nine had been considered and rejected, or had rejected him, he betook himself to a lady friend for advice, and she produced number ten. Kepler paid his first visit, but he tells us, "She has undoubtedly a good fortune, is of good family, and of economical habits, but her physiognomy is most horribly ugly,—she would be stared at in the streets; not to mention the striking disproportion in our figures. I am lank, lean, and spare; she is short and thick; in a family notorious for fatness, she is considered superfluously fat." Number eleven seems to have been too young.

At last Kepler, after much searching of heart, resolved to choose one he had thought of before, but whom his friends had persuaded him to pass by on account of her humble station, and this is his description of her: "Her name is Susanna, the daughter of John Reuthinger and Barbara, citizens of the town of Eferdingen. . . . She has received an education well worth the largest dowry. . . . Her person and manners are suitable to mine—no pride, no extravagance; she has a tolerable knowledge how to manage a family; middle-aged, and of a disposition and capability to acquire what she still wants. Her I shall marry, by favour of the noble Baron of Strahrenberg, at twelve o'clock on the 30th of next October, with all Eferdingen assembled to meet us, and we shall eat the marriage dinner at Maurice's, at the Golden Lion."

In 1612 Rudolph died, and was succeeded by his brother Mathias, who confirmed Kepler in his appointment of Imperial Mathematician, and gave further orders for the payment of his arrears of salary—which were as usual neglected; but he allowed him to accept the professorship at Linz.

Kepler now had some small relief from his pecuniary difficulties, but he suffered from new troubles. The Catholics of Linz were specially bigoted, and Kepler was excommunicated. "The priest and the school inspector," he says, "combined to brand me with the public stigma of heresy, because in every question I take that side which seems to me to be consonant with the Word of God." In 1617 he refused the offer of a professorship at Bologna, the salary of which would have been wealth compared to his present income, because, as he said, "From a boy up to my present years, living a German among Germans, I am accustomed to a degree of freedom in my speech and manners which seem likely, if persevered in on my removal to Bologna, to draw upon me, if not danger, at least notoriety, and might expose me to suspicion and party malice."

About this time he published books on gauging and harmonics, and was summoned to Ratisbon, to give his opinion on the reformation of the calendar, by the government that neglected to pay him his salary.

In 1619 he published his book, "On the Harmonies of the World," dedicated to James I. of England. In this was announced his third law. With an intuition that we can but faintly understand, he had seen that if only he could discover some relation between the space characteristics and the time characteristics of the planets, he would therein hold wrapped up the key that would unlock the mystery of physical astronomy. He had found this connection for each planet separately in his second law. There, a relation between the rate of motion of each planet at any instant and its position in its orbit at that instant is stated; it remained to find a relation between the average rate of motion of any planet and the position of its orbit. We have no space to describe the endless theories that were tried and rejected, and the laborious calculations in which he was involved. The result he ultimately arrived at was that "The cubes of the mean distances of the planets from the sun are to one another as the squares of the times they take to describe their orbits." His delight at this discovery of what he had been seeking all his life was unbounded.

In this same year, 1619, Mathias died, and was succeeded by Ferdinand III., who again promised to pay the arrears of Kepler's salary; and some small part of it was paid. This enabled Kepler to carry out the work he owed to the memory of his friend and benefactor, Tycho, by the publication of the Rudolphine Tables. He had had them by him completed for many years, and at last, in 1621, they were given to the world.

In 1622 was published the "Epitome of the Copernikan Astronomy," which was immediately placed by the Church in the index of forbidden books. In this year Kepler lost his violent old virago of a mother. She had been some years before charged with witchcraft and administering drugs to an acquaintance. Her accuser was Christopher Kepler, one of her own two worthless sons whom she petted while she ill treated John. In 1620 she was sent to prison and condemned to torture. Her son, hearing of this, set out at once from Linz, and arrived just in time to save her from punishment. She was ultimately released; but not content with this, the fierce old woman, though in her seventy-ninth year, raised an action against her accusers, which was stopped by her death. Let us endeavour to say what we can in favour even of her. She was at least honest. Kepler had been educated at school at the expense of the Duke of Wirtemberg; but his mother thought that his college instruction was still unpaid for. Mœstlin writes to Kepler, "Your mother had taken it into her head that you owed me two hundred florins, and brought fifteen florins and a chandelier towards reducing the debt."

In 1629 Kepler received an invitation from Albert Wallenstein, Duke of Friedland, to take up his abode at Sagan, under his protection. This the needy astronomer eagerly accepted, and he was given a professorship at Rostock as well. It now seemed as if good fortune were in store for him; but he did not live to enjoy it. For on a journey to Ratisbon, to make one final effort to get his arrears paid, he caught a fever. Overstudy brought on brain disease, and on the 5th of November, 1630, he died.

The constant ill health and extreme poverty in which he carried on his stupendous labours render the story of his life a tragedy; but these were only a small part of his sorrows. We have seen with what scorn he looked upon what he calls "the filthy astrological superstitions of that vulgar and childish race of dreamers—the prognosticators;" we have seen something of that single-minded earnestness of his search for truth which has earned for him the character of "the honestest man that ever lived;" we can therefore realize to some extent his misery when, in order to win bread for his children, he was forced to write and sell what he himself calls "a vile, prophesying almanack." The world would not give him bread for his splendid astronomical discoveries, but it would give him money for what he knew to be lies.

Yet let us not presume to pity him. He had an intellectual meat to eat whereof we can know but little. He could peer deep down into the secret springs and hidden bases of nature, and there he saw that if he could find a relation between the space characteristics and the time characteristics of the planets, he would possess the key to the whole mystery. To find this was the great aim of his life, and we shall carry away a truer notion of his fortunes if we take leave of him at that supreme moment when he grasped the visionary object of his early labours, the crown of his later toil, and burst into a passion of joy. "What I prophesied two and twenty years ago, as soon as I had discovered the five solids among the heavenly bodies; what I firmly believed before I had seen the 'Harmonies' of Ptolemy; what I promised my friends in the title of this book, which I named before I was sure of my discovery; what sixteen years ago I urged as a thing to be sought; that for which

I joined Tycho Brahé, for which I settled in Prague; for which I have devoted the best part of my life to astronomical contemplations;—at length I have brought to light, and have recognized its truth beyond my most sanguine expectations. . . . It is now eighteen months since I got the first glimmer of light; three months since the dawn; a very few days since the unveiled sun, most beauteous to behold, burst out upon me. Nothing holds me. I will indulge in my sacred fury. I will triumph over mankind by the honest confession that I have stolen the golden vases of the Egyptians, to rear up a tabernacle to my God far away from the confines of Egypt. If you forgive me, I rejoice; if you are angry, I can bear it. For the die is cast, the book is written, to be read now or by posterity; I care not. I can well wait a century for a reader, since God has waited six thousand years for a discoverer."

CHAPTER V.

ON GALILEO AND THE LAWS OF MOTION.

So far we have considered the work of Copernik, whose struggle was with the Aristotelian slavery in which his own mind was bound; of Tycho Brahé, who had to fight against the social prejudices of himself and his family; of Kepler, whose life was spent in poverty, and whose work was to some extent impeded by the religious hostility of the Catholics. We now have to consider the work of a man whose whole life was embittered by this hostility, and who at last died in misfortune caused by it.

We have seen that the great treatise of Copernik was dedicated to a pope, and published at the expense of a cardinal. It is obvious, therefore, that the ideas contained in it were not at first viewed with displeasure by the Church. But as they grew in influence, they naturally excited the opposition of the Aristotelians; and it was not long before the intellectual authority of the Aristotelians came to be regarded as the necessary consequence of the religious authority of the Church. For, before the days of Copernik, earnest men had deeply felt the religious decrepitude of the Church, as then evidenced in the immorality of her ministers ; and when the Renaissance had come, with the overthrow of the idea of authority in intellectual things, this moral indignation ripened into the overthrow of the idea of authority in spiritual things. Hence arose the Reformation ; and the Church naturally regarded with hostility that intellectual independence which had led to freedom from her spiritual authority, and so her ministers considered themselves bound to be in intellectual matters submissive to the hitherto unquestioned authority of Aristotle.

During the sixteenth century greater earnestness grew up in the Church itself, and, with that, greater strength. She purified herself from the immorality which defiled her, and throughout that century she regained much of what she had lost at the first outburst of the Reformation. At the time, therefore, of which we are speaking, the Church possessed both the power and the will to crush out, in the greater part of Western Christendom, all belief in the truths of science ; and so we find followers of Copernik among the martyrs.

Peter Ramus (born 1502), who had been a shepherd lad, but was sent to the University of Paris, argued against the authority of Aristotle, and was slain in the Massacre of St. Bartholomew. And Bruno of Nola, the friend and guest of Sir Philip Sidney, powerfully attacked the Aristotelians, and was condemned as a heretic, and burnt to ashes at the stake at Rome in the year 1600.

The man whose work we are about to consider lived in Italy all his life, and so was peculiarly liable to the constant pressure of the Church. But he has been almost as perversely treated by posterity as he was by his contemporaries. He is popularly supposed to have invented the telescope; but one does not often hear references to the fact that he discovered the laws of motion, and probably most people would imagine it to be a far greater thing to invent the telescope than to discover the laws of motion. As a matter of fact, the invention of the telescope was a happy accident, while human genius has probably never been more strikingly displayed than in the discovery of the laws of motion; and Galileo did not invent the telescope, but he did discover the laws of motion. Besides this great work, however, his telescopic observations on the stars and planets verified the Copernikan system, and give the "holding turn" to the growing belief in it.

It is a remarkable fact that the same city produced the great poet who first began to build up modern literature amid the ruins of the Greek and Roman civilizations, and the great philosopher who led the forlorn hope of the new science against the old. For Galileo Galilei, though born at Pisa, on the

15th of February, 1564, was of an ancient Florentine family. His father, Vincenzo Galilei, was a man of learning, and was famous as a writer on the theory of music. The family was not wealthy, and so the young Galileo was intended to be a cloth merchant, an occupation which not only brought in wealth, but was much thought of among the Florentines at that time.

With this view, after some elementary instruction in the classical languages, the future astronomer was sent to school at the Convent of Vallòmbrosa, where he laid the foundation of that beautiful literary style which served him in such good stead in his controversies with the "paper philosophers," as he called the Aristotelians. At this school he made such progress in all his studies that it was determined he should adopt some profession instead of setting up in a trade, and that of medicine was chosen. With this view he entered the University of Pisa when he was seventeen years old, on the 5th of November, 1581.

At the university, a very short study of philosophy convinced Galileo of the absurdity of the *a priori* methods of reasoning on scientific subjects indulged in by the Aristotelians; and he vigorously attacked and ridiculed the authority of Aristotle on every occasion. This earned for him the displeasure of the professors, and the nickname of "The Wrangler" among the students. So far, however, he seems to have given no special attention to scientific subjects, except perhaps in his boyhood, when he used to amuse himself with making working models of machines. It is even said that at the age of nineteen he was absolutely ignorant of mathematics; but about that time, while he was studying medicine, the court of Tuscany came to Pisa for some months. Among them was Ostilio Ricci, an old friend of the Galilei family. This man was a good mathematician, and was instructor of the pages. One morning Galileo was present at a lecture by him on Euclid, and, for some time after that, used to come every day with his Euclid and listen to Ostilio's explanations from behind the door. At last he mustered up courage, and begged for further instruction, which was freely given.

Vincenzo Galilei was extremely displeased to hear of this rival to his son's medical studies, and with great reluctance gave his consent to the arrangement. It had been difficult for Vincenzo to maintain his son at the university, and he now applied to Ferdinand de' Medici, the reigning grand-duke, for one of the forty free places at his disposal in the university. The application was refused, owing to the influence of the Aristotelian professors, and, after four years at the university, Galileo was obliged from want of money to leave without taking a degree.

At home he continued his scientific researches, and in 1587 we find him in Rome, where he made the acquaintance of various distinguished men, with whom he began to correspond, such as Clavius, Riccoboni, the Marquis Guidubaldo del Monte, etc.

The vigour and clearness of his reasoning, and the invention of his "hydrostatic scales," so impressed these men, that Galileo found himself becoming famous; and in 1589, Del Monte, who had given him the title of "the Archimedes of his time," procured for him the professorship of mathematics at Pisa, with an income of about £13 a year, for three years. This put him in a position in which he could earn a living by giving private lessons.

Here he began his more direct researches into the laws of motion, which we shall consider more particularly further on. These researches involved the experimental testing of some of Aristotle's assertions, and the complete refutation of them that ensued, instead of convincing, only exasperated the "paper philosophers."

An illegitimate son of the half-brother of the reigning grand-duke had some superficial knowledge of mechanics, and invented a machine for cleaning the harbour of Leghorn; Galileo was commissioned to examine this instrument, and reported that it was useless. On trial this was found to be true, and the disappointed inventor began an intrigue with the Aristotelians against the mathematician. Galileo, hearing of this, and knowing he could not hope to stand against their malice, resigned his professorship, and went home to Florence.

On the 2nd of July, 1591, his father died, and left the support of his family of one son and three daughters to Galileo. He was now in very poor circumstances, but the Marquis del Monte again came to his help, and induced the Senate of the Republic of Venice to confer on him the professorship of mathematics at Padua for six years, with a salary of £18, which was gradually raised to about £100 a year. He entered upon his work there on the 7th of December, 1592, with an inaugural address, which impressed his audience as much by its entrancing eloquence as by its scientific ability.

Galileo was now in the full tide of his scientific work. He wrote treatises on the laws of motion, on fortification, on the making of sun-dials, and on the celestial globe; crowds of students flocked to his lectures; and he served the Government of Venice by the invention of various useful machines. Several of these inventions and treatises were dishonestly claimed by others, and in a vindication of the priority of his work over that of Balthasar Capra, of Milan, he exhibited for the first time his great polemical powers.

It is interesting to know that about this time some of Kepler's too violent partisans attacked Galileo, with the assertion that he plagiarized some of their master's discoveries; but Kepler severely rebuked them, and apologized to Galileo in the warmest terms for their misconduct. A base attempt to ruin him by secretly accusing him to the Government of immorality of life, was only met by the refusal of the Senate even to investigate the false charges, and by their increasing his salary.

While a young man, Galileo enjoyed a strong constitution, but in the year 1594, he chanced to sleep one afternoon in a room artificially cooled by a current of air which passed through falling water. He thereby contracted a chronic complaint which never left him, and which kept him for the rest of his life in almost continuous physical pain.

It was while professor at Padua, in the year 1609, that Galileo heard of the invention of the telescope by Hans Lipperhey, of Middleburg. He at once set to work to inquire how such an instrument could cause objects to appear nearer than they were, and he was probably the first to give a scientific explanation of the phenomenon. He then constructed an instrument which would cause objects to appear three times nearer than they were, and afterwards he made one which would bring them in appearance to one-thirtieth of their actual distance. He had the honour of publicly exhibiting and explaining this telescope to the Senate at Venice. Ultimately he presented it to them, and in return was confirmed in his professorship for life.

Galileo had throughout his life at Padua longed to find employment under his own Government at Florence, and he had been in the habit of spending his vacations at the court of Tuscany, and giving instruction to Cosmo, the heir to the dukedom. When this prince succeeded, Galileo applied for some employment under his Government, and was ultimately appointed chief mathematical professor at Pisa, with a salary of about £220 a year, and with no obligation to lecture. He left Padua, greatly to the grief of all members of the university, in September, 1610.

This transaction, which does not say much for his gratitude to the Government that eighteen years before had befriended him when he was in poverty, was the first great mistake of his life, and became, as Libri says, "the beginning of all his misfortunes." He seems to have been urged to it partly by a desire to return to his native place, and partly by the desire to obtain freedom from lecturing, in order to devote himself entirely to research; but in leaving Padua he left the free soil of a republic for the fettered dominion of a despotism subject to the Church. Before, however, we go on to consider the progress of his persecution, we must examine more carefully the discoveries he had been making all these years.

Galileo seems to have been converted to the Copernikan system in the early part of his first professorship at Pisa, but the exact date and occasion of it is unknown. Voss, an inaccurate writer, asserts that a lecture by Mœstlin, which he chanced to hear, first convinced him; but Galileo himself seems to hint that a short course of lectures by one Christian Wurteisen, of Rostock, was the occasion. At any rate, it is pretty clear that he continued to teach the old Ptolemaic system after he had become convinced of its untruth, and even after he had been appointed professor at Padua; but the appearance of the new star of 1604 made him attack the Aristotelian assertion of the unchangeableness of the "primum mobile," and the revelations made to him when he turned his telescope upon the heavenly bodies soon drove him to overthrow the whole of the ancient system.

One of the very first facts revealed by his telescope was that it in no way altered the appearance of the fixed stars, while the planets were magnified by it into discs instead of mere points of light. As there could be no doubt that the instrument really did have the same effect upon the appearance of the fixed stars as it had upon the planets, the only explanation that could be given was that the fixed stars were at so vast a distance that, when brought thirty times nearer, they still appeared as mere points of light, while the planets must be much nearer. This removed one great objection to the Copernikan system—the fact that, if the earth revolved round the sun, the fixed stars must be at an enormous distance compared with the distance of the earth from the sun; compared, that is, with the dimensions of the solar system, or, which is the same thing, with the distances of the planets from the earth. Men had now a new and entirely independent reason for believing that this was actually the case, and hence the probability of the Copernikan system was increased.

But a further verification was soon made. The first planet Galileo examined with his telescope was the planet Jupiter. He at once noticed three small stars, invisible to the unassisted eye, and nearly in a line with the planet and with the ecliptic. On looking at them the next night, he was surprised to find that they had altered their positions and that a fourth one appeared. Watching them carefully night after night, he found that each of them appeared to move alternately to one side and to the other of the planet, always keeping nearly in the ecliptic. He could not doubt that these bodies—which he called the Medicæan stars, in honour of Cosmo—were in reality revolving round Jupiter in the plane of the ecliptic, and their orbital motion, being therefore seen edgeways, appeared to be rectilinear. Here, then, men saw actually going on in the sky an image of what Copernic asserted was the motion of the whole solar system; and inasmuch as men's beliefs depend much more upon familiarity with the ideas believed in than upon reason, this discovery did much to popularize belief in the true system of astronomy.

The excitement caused in the world by these discoveries was immense. Most people refused to believe them at first; the "paper philosophers" continued in their stupid disbelief, and obstinately refused to look through the telescope, answering Galileo's observations with texts from Aristotle. It is interesting to find, however, that Kepler at once believed, even though the discovery seemed to overthrow the discovery of "the five solids among the heavenly bodies," which he imagined he had made. Galileo writes to him, "You are the first and almost the only person who, even after but a cursory investigation, has—such is your openness of mind and lofty genius—given entire credit to my statements. . . . Why are you not here? What shouts of laughter we should have at this glorious folly! and to hear the Professor of Philosophy at Pisa labouring before the grand-duke with logical arguments, as if with magical incantations to charm the new planets out of the sky."

Galileo's observations on the moon showed him that her surface was rough and crossed by hills and valleys, and still further incensed the Aristotelians, who regarded it as smooth like a mirror.

His next discovery was announced to the world in an anagram, as follows:—"Smaismrmilme poeta leumi bune nvgttaviras;" which was puzzled at in vain until the discoverer deciphered it into the words, "Altissimum planetam tergeminum observavi"—"I have observed the most distant planet to be triple." He thought he had seen Saturn as three stars—a large one in the centre and a smaller one on each side. But this discovery caused him some trouble, for, some time after, the two attendant stars disappeared. The phenomenon was not completely explained until Huygens, in 1656, using a better instrument, found Saturn to be surrounded by a flat ring, which was not exactly in the plane of the ecliptic, and so was sometimes visible in the form of an oval, but when looked at edgeways it was so thin as to be invisible.

In Galileo's explanation of his anagram, he declared that nothing remarkable was to be seen in the other planets. But in less than a month another discovery was announced in another anagram, "Hæc immatura à me jam frustra leguntur oy;" "These unripe facts are now vainly gathered by me," which, being transposed, may be read, "Cynthiæ figuras æmulatur mater amorum,"—"The mother of the loves (Venus) rivals the appearances of Cynthia (the moon)." The wonderful prophecy of Copernik had been fulfilled; the sense of sight was rendered sufficiently powerful, and phases were seen in Venus. There could now be no doubt that that part of the Copernikan system which asserted that Venus revolved round the sun was true.

Still the great objection to the system remained. Men could not yet bring themselves to believe that this firm and solid earth was spinning round like a top. It was reserved for our own time for experiments to be devised by which we can, so to speak, see the earth rotating;* but another observation of Galileo removed some of the prejudice against this idea. Spots had been more than once observed on the sun's disc centuries before this time, but they were always supposed to be either Mercury or Venus passing between him and the earth. The more accurate observations of Galileo proved that this could not be the case. He showed that they moved too slowly across the sun's disc, and moreover they always moved in the same direction and took the same time to cross his disc whether the chord of the circle along which they

* *Vide* p. 298.

moved were short or long, and they invariably were flattened in the direction perpendicular to the edge when they approached it. There could therefore be no doubt that they were actual spots on the surface of the sun himself, which were carried round by his rotation. It was known that the sun was an enormously large body, and when men knew that he rotated, it was easier for them to believe that the earth also was rotating.

If the moon had no motion of rotation, an observer on the earth would pass through every possible direction from her in the plane of her orbit once in the course of a single lunar revolution, but she rotates on her axis at a uniform rate once while she completes a revolution round the earth; therefore, if she moved so that the line joining her centre to the centre of the earth revolved at a uniform rate, she would always present exactly the same face to the centre of the earth. This she very nearly does, though we shall have to notice a modification of this general statement in the next chapter. Assuming, however, for the present that this is true, we see that in the course of half a day, from moonrise to moonset, the observer is carried from being the radius of the earth distant from the line joining the centres of the earth and moon on one side of it to the same distance on the other side of it, and hence he can look a little round her edge on one side while she is rising and a little round the other side while she is setting, so that the moon appears to turn a little from side to side in the course of a day. This phenomenon, which is called the moon's "diurnal libration," was first noticed by Galileo, and was the last of his astronomical observations.

The discoveries we have so far referred to formed, no doubt, that part of his work which was most striking to his contemporaries. But his most important work had to do, not with completing the past, but with stretching forward into the future.

It is obvious that, before we can theorize upon physical astronomy, we must understand the laws of motion; before we can investigate what mechanism produces and maintains the movements of the heavenly bodies, we must know what effect forces of given magnitude applied in a given direction would have upon a body in motion; and before we can know what effect applied forces would have upon moving bodies, we must know what the natural state of motion of a body is, how it would move if started and then left entirely to itself without any forces acting upon it. We have seen what the Greeks thought about this last question: they fancied that the natural state of motion of a body was motion at a uniform rate in a circle; and to a certain extent this theory accounted for the facts of astronomy, if a mechanism of epicycles were assumed. We have seen what Copernik thought; but we have seen that if his complicated notion of the two sorts of motion, one belonging to whole bodies, and the other belonging to the separated parts of whole bodies, be true, the motion of a falling stone near the earth's surface is inconsistent with the earth's rotation. We have not yet seen what Kepler thought, for he nowhere distinctly formulates his views; but there can be no doubt, from his idea of spokes pushing the planets round, that he thought that a body when started and left to itself would in time stop; and this idea, though wrong, appears at first sight consistent with all experience.

The science which treats of forces is called dynamics, and consists of two parts—statics, which is concerned with forces acting upon a body in equilibrium; and kinetics, which has to do with forces acting upon a moving body, and changing its motion. In astronomy we have only incidentally to do with statics. Its fundamental principles had been rightly ascertained long before this time by Archimedes of Syracuse (born B.C. 287, died 212) and Simon Stevin of Bruges (born 1548, died 1620); and these led men's minds to the consideration of kinetics, but nothing was discovered in this science until Galileo began his investigations.

A remark of Benedetti's (about 1585) is, however, worth mentioning. Referring to Aristotle's difficulty to account for the fact that a stone when thrown retains some of its motion after the cause of its motion—the throwing arm—has been withdrawn, he says, "The motion of the body, separately from the mover, arises by a certain natural impression from the impetuosity received from the mover." He further states that this impetuosity increases when the body is moving towards the place assigned to it by nature—in the case of bodies near the earth's surface, the centre of the earth—but diminishes otherwise, and he notices that the air impedes all sorts of motion. Galileo's first work was to show that this resistance of the air and other resistances, such as friction, to which it might be subjected, were wholly sufficient to account for all the loss of "impetuosity" in bodies projected horizontally.

This is the essential principle of Galileo's first discovery, which was afterwards stated in its clearest and simplest terms by Newton, in his "Principia," as the first law of motion.

"Every body continues in its state of rest or of uniform motion in a straight line, except in so far as it may be compelled by impressed forces to change that state."

That a body at rest would remain at rest if left to itself, no one ever doubted; but the rest of the law is of the utmost importance, and involves two statements. *First*, that the natural free state of motion of a body is uniform motion in a straight line; and *secondly*, that anything that changes that state of uniform motion in a straight line is a force. It seems a very easy thing to see the truth of this law when once it is pointed out; but probably nothing was more difficult than to discover it, for the simple reason that it is never seen in independent operation, for we shall see in the next chapter that Newton discovered that every particle of matter in the universe exerts a force on every other particle. The law itself was no doubt suggested by the fact that the more we free a body from the action of forces the more nearly will its motion approximate to uniform motion in a straight line. If, for instance, we project a stone on a flat horizontal surface, so as to counteract the effect of the force of gravity on it, it will move in a straight line, and the smoother we make the surface the longer it will continue to move before being brought to rest. But the ground for believing in the truth of the law is the fact that the predictions of the science of dynamics, which are mathematical deductions from three laws of which this is one, have been found after a long and elaborate course of verification, and are still found invariably, to agree with the facts.

Having now found what is the natural free state of motion of a body, we must go on to find how forces are to be measured, and what their effects are when acting upon a body in motion.

If a body be left at rest in mid-air, it will fall; hence, according to the first law of motion, some force must act upon it. If the body be placed to rest upon a spring, the spring will be deflected to the same sensible extent wherever it and the body be placed; hence this force, which is called gravity, was supposed to be a *uniform* force. It remained to be seen what effect this force, uniform when measured in this statical manner, would have upon a body in motion.

It had been observed before the time of Galileo that a body resting on a smooth inclined plane required a less force to support it than the same body suspended in mid-air; and, moreover, that the force needed was the same at whatever part of the inclined plane it rested. A body allowed to slide down an inclined plane would, therefore, be acted upon by a small uniform or "constant" force, and it was found consequently to move under the action of that force, but slowly, and hence its motion could be observed. The first idea that Galileo seems to have had was that "the swiftness of the movable increases in the proportion of its distance from the point whence it began to move." This, however, he soon found to be logically inconsistent with his observation that "the spaces passed (from rest) . . . are in double proportion (that is, as the squares) of the times." That is to say, if a body would slide one foot down the inclined plane in one second, in two seconds it would slide four feet, in three seconds nine feet, and so on.

This, then, is the effect of a constant force. It is mathematically deducible that a body moving in this way will have equal increments of velocity added to its motion in equal times; that is to say, in the instance just taken, at the end of one second the body would have a velocity of two feet a second; that is, if it continued to move with the velocity it had at the end of the first second, it would, in the next second, move through two feet. But the force is constantly adding on velocity, and during the second second the body will have added to its motion a velocity of two feet a second; so that after two seconds from rest it will be moving with a velocity of four feet a second; similarly, at the end of three seconds it will have a velocity of six feet a second, and so on. A constant force may be therefore defined kinetically as a force which adds on the same increment of velocity in the same time, and may be measured, if we always consider the same body, by the magnitude of the increment of velocity it adds on in a unit of time. It is important to notice that this velocity is added on continuously, and not by jerks; so that in the illustration above, in one-thousandth of a second, a velocity of one-thousandth of two feet a second would be added on, that is, a velocity of one five-hundredth of a foot a second.

So far we have considered the same body moved by the same or different forces; but what happens if we consider different bodies? A body, which we will call A, weighing a hundred pounds, is ten times as heavy as a body B, which weighs only ten pounds, and requires ten times the force to support it in mid-air; therefore the force of gravity on it, which is constant in all its positions, is ten times as great as the force of gravity on B. Aristotle rashly asserted that under these circumstances, if A and B were dropped from the same height on to a horizontal plane, B would take ten times as long to fall as A would. But one of Galileo's first contests with the Aristotelians was over his experiments on bodies falling from the top of the leaning tower of Pisa, in which he showed that all bodies take the same time to fall. This seems inconsistent with the varying force on different bodies; but the explanation is that a body that weighs ten times as much as another contains ten times as much matter, or is ten times the "mass" of the other, and therefore is ten times as much to move, and hence requires a force ten times as great to move it at the same rate. The force on a moving body must, therefore, be considered as proportional conjointly to the mass of the body and the number of units of velocity added to its motion in a unit of time.

The number of units of mass of the body, multiplied by the number of units of velocity it possesses at any instant, is the measure of what Newton afterwards called the "quantity of motion" of the body at that instant—what we should now call its "momentum." Galileo found that every force had its full effect adding on quantity of motion in the direction in which it acts, no matter what velocity the body has, or what is being produced in it at the same time by other forces; the exact meaning of this latter discovery we shall consider hereafter. Meantime we may state this second law of motion in the form Newton afterwards gave it—

"Change of motion (that is, of 'quantity of motion') is proportional to the impressed force, and takes place in the direction of the straight line in which the force acts."

The investigation of these laws was spread over the whole of Galileo's life, and was completed in the last book he ever wrote. We must now take up his personal history at the moment he arrived at the court of Cosmo, in 1610.

Paul V. had placed Venice under an interdict, which proceeding was answered by the expulsion of the Jesuits from the territory of the republic in 1606. It was well known that Galileo sympathized with this act, and hence the Jesuits leagued with the Aristotelian professors to ruin him. Galileo suspected this, and obtained leave from the Florentine Government to go to Rome, in order at once to vindicate his opinions. He was received with such magnificence and good will that his enemies were discomfited. They accordingly determined to adopt more subtle methods. They induced a Dominican priest named Caccini to accuse the Copernikan system of being opposed to Holy Scripture. This fellow preached a sermon with the text, "Ye men of Galilee, why stand ye gazing up into heaven?" and in it violently abused Galileo. Galileo fell into the trap, and in reply powerfully argued that the Bible was never intended to teach physics. He was then secretly denounced to the Inquisition.

In 1615 he again went to Rome, and conducted a series of discussions on the Copernikan system with the most brilliant success. The Inquisition at last took the matter up, and the Copernikan system was condemned as "foolish and absurd philosophically and formally heretical;" and Galileo, though not forbidden to teach it as a mere hypothesis, was admonished to be careful how he treated it. Unfortunately, he was outwardly submissive. Personally he was unmolested, and he returned home on the 4th of June, 1616.

Seven years passed quietly. Cosmo was dead, and the government of the grand-duchess, as regent for Ferdinand II., was in the hands of the Jesuits. Paul V. and his successor Gregory XV. had died, and Urban VIII., who as Cardinal Barberini had been Galileo's friend, was pope. Galileo now returned to Rome, having, as was announced to Urban, "an ardent desire to kiss his toe, if his Holiness would permit it;" but though "his Holiness" graciously permitted him to kiss his toe, he would not rescind the condemnation of the Copernikan system.

In February, 1632, Galileo published at Florence one of his greatest works, his "Dialogues on the Two Principal Systems of the World, the Ptolemaic and Copernikan." In this work the arguments for the true system are put forward with the utmost power, and the formal submission to the papal decrees only added by its bitter sarcasm to the fury of the Jesuits. They now adopted a discreditable trick to ruin the author. They persuaded Urban that one of the characters in the "Dialogues," Simplicius, who always appears in a ridiculous light, was intended for himself. The pope's mind was poisoned against Galileo, and he appointed commissioners to institute a trial of him for heresy. But there was no pretext for the charge, and so a document was forged purporting to be a positive prohibition to Galileo, by the pope and the Holy Office, sixteen years before, to teach the Copernikan system.

We have no space to enter here upon the revolting details of this trial, Galileo's growing terror, his increasing ill health weakening body and mind, his successive examinations by the Inquisition, the threat of torture, and the final pitiful submission of the broken-down old man. The long sentence pronounced on Wednesday, the 22nd of June, 1633, which no honest man can read with patience, full as it is of the most blasphemous assumption of divine authority by his judges, ends with the words, "We condemn you to the formal prison of this Holy Office during our pleasure, and by way of salutary penance we enjoin that for three years to come you repeat once a week the seven penitential psalms"! Let us, however, not omit to do an act of justice. It is refreshing to find such a thing as a good man even among the Inquisitors. Three among his ten judges—the Cardinals Caspar Borgia, Laudivio Zacchia, and Francesco Barberini, the pope's nephew—refused their assent to the infamous sentence.

It does not appear that Galileo was ever tortured or that he was specially ill treated in prison. But he was detained in confinement at Rome. Here his health suffered during the heat of summer, and his request to be allowed to return to Florence was refused. At last he was permitted to go to his villa at Arcetri, where nearly two centuries and a half afterwards—in 1872—the royal observatory of his emancipated nation was erected. Every relaxation of his misery excited the malice of his enemies; secret denunciations of his influence were poured into Rome, and kept him in constant fear. Then his favourite daughter died. Yet he roused himself out of even this misfortune, and wrote his work on motion. At last his final misery overtook him. He was attacked by a disease of one eye, which slowly darkened, and then the other began to fail, and at last he was quite blind. "This heaven, this earth," he wrote, "this universe, which with wonderful observations I had enlarged a hundred, a thousand times beyond the belief of bygone ages, henceforth for me is shrunk into the narrow space which I myself fill in it. So it pleases God; it shall therefore please me also." "The noblest eye," wrote Father Castelli, "that nature ever made is darkened; an eye so privileged, and gifted with such rare powers, that it may truly be said to have seen more than the eyes of all that are gone, and to have opened the eyes of all that are to come."

Galileo was now at Florence, but in rigorous confinement. The few who were allowed to see him were forbidden to discuss the Copernikan system with him. At last he was sent back to Arcetri, and there, after three years of life in physical misery though with unabated strength of intellect, on the 8th of January, 1642, he died.

During these latter years and for some time afterwards the Copernikan system was at a discount in

Rome. Many learned books were published, completely refuting it, with the approval of infallible authority. We will quote a passage from one of these as a specimen of their arguments. "Animals which move have limbs and muscles; the earth has no limbs or muscles, therefore it does not move. It is angels who make Saturn, Jupiter, the sun, etc., turn round. If the earth revolves, it must also have an angel in the centre to set it in motion; but only devils live there, it would therefore be a devil who would impart motion to the earth. The planets, the sun, the fixed stars, all belong to one species, namely, that of stars; they therefore all move or all stand still. It seems, therefore, to be a grievous wrong to place the earth, which is a sink of impurity, among the heavenly bodies, which are pure and divine things." *

The Church had succeeded in stifling the brightest light that God set up in those days to illumine this dark world, but she was in the end powerless against the truth. As the centuries rolled on, even Jesuits came to believe the truths of science, and one by one books were allowed which assumed or supported the Copernikan system. Still the great work of Copernik himself remained condemned. At last, in the new edition of the "Index," which appeared in 1835, after two hundred and nineteen years of infallible stupidity, "De Revolutionibus Orbium Cœlestium" was quietly omitted from the list.

* Quoted in "Galileo Galilei and the Roman Curia," p. 271.

CHAPTER VI.

ON NEWTON AND THE DISCOVERY OF THE LAW OF GRAVITATION.

WE may now leave the stifling atmosphere of ecclesiastical despotism, and consider the progress of astronomy in the bracing air of Protestant England.

The Copernikan system was probably introduced into this country by Bruno of Nola, who has left in his works accounts of discussions in which he defended it, while staying with Sir Philip Sidney, in London. Bacon, however, refused to believe in it, and it is interesting to see the grounds on which he took this course. In his "Description of the Intellectual Globe," he says that up to his time astronomy had been limited to the investigation of the rules of the heavenly movements, and philosophy to the investigation of their causes, and that these had been regarded as entirely separate; but that if men would discover the truth, they must recognize that these two studies are "one and the same thing, and compose one body of science." He would give his adherence to no formal theory until the problem of physical astronomy had been solved. And it is interesting to know that this problem was worked at in England far more than in any other country.

With the exception of Bacon, however, English men of science seem to have adopted the Copernikan system soon after its introduction. Bishop Wilkins, by his amusing extravagances, probably directed attention to it. Gilbert, the investigator of magnetism, accepted it, and all the English mathematicians—Napier, Briggs, Horrox, Crabtree, Oughtred, Ward, Wallis, and Wren were Copernikans.

Milton, when he wrote "Paradise Lost,"* seems to have been still undecided, though the clearness with which he there describes Copernik's hypothesis seems to imply that he leaned to the new system. And indeed all his religious and political convictions would tend to lead him in this direction; for we read in the "Areopagitica" of his travels in Italy, "There it was that I found and visited the famous Galileo, grown old, a prisoner to the Inquisition for thinking in astronomy otherwise than the Franciscan and Dominican licensers thought." †

In physical astronomy we have hitherto noticed only one theory—that of spokes issuing from the

* *Vide* beginning of bk. viii.

† Bohn's one-vol. edit. of Milton's Works, p. 112.

sun, and pushing the planets round in their courses, suggested by Kepler. One other must be mentioned, of great historical celebrity, though of no scientific value whatever—that of vortices, invented and maintained by Descartes.

Noticing the same facts observed by Kepler, namely, that the planets all move round the sun in the same direction, from west to east, in orbits which are very round ellipses, nearly circles, and nearly in the same plane, and that the sun rotates on his axis in this same direction, Descartes suggested that all space was filled with air, in which whirlpools and vortices were set up ; that one great vortex existed round the sun, carrying the planets in it ; and just as he observed a whirlpool in a river to rotate faster the nearer the particle of it considered is to the centre, so by this means the planets that are nearer to the sun would revolve faster than those more remote. An unsatisfactory explanation was attempted of the way in which subsidiary vortices might be set up in the primary vortex, to account for the movements of the satellites ; and an equally unsatisfactory explanation was attempted of the way in which one planet might push another a little out of its circular path, to account for the elliptical form of its orbit. The great mathematical and philosophical reputation of Descartes gained for this theory a currency far beyond its intrinsic merits, and Leibnitz and John Bernoulli even defended a modification of it against Newton's theory of gravitation.

But before we can explain the essential error in both Kepler's and Descartes's theories, it will be necessary to examine more carefully the meaning of Galileo's second law of motion.

He was led to it by considering the motion of projectiles, and in particular by observing that, if a body be projected from any point above a horizontal plane, it will fall on to the plane in exactly the same time as if it be dropped from rest at the point. In this case, then, the vertical velocity added on by gravity is entirely independent of the horizontal velocity with which the body is projected. And this is found to be true universally: that any number of velocities communicated to a body have each their full effect, so that the combined result on the motion of a body at any instant of a number of velocities communicated to it by a number of causes acting simultaneously may be found by finding what the effect due to each velocity would be if it acted separately, and then finding what the effect upon the body would be if these effects be allowed to take place successively.

From this an extremely important geometrical proposition relating to motion follows. This proposition, which is called the parallelogram of velocities, may be proved in the following manner:—

Let a body be placed at A (Fig. 9), and let two velocities be simultaneously communicated to it, one of which would cause it to move from A to B in a second of time if it were communicated alone, the other of which would cause it to move from

A to D in a second if communicated alone; the question is—What will happen if the two be communicated simultaneously? According to this second law of motion, they will each have their full effect; that is, the result will be the same as if they were com-

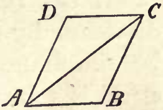

FIG. 9.

municated successively. Suppose them, then, to be communicated successively. If the first act alone, the body will be carried from A to B in a second of time. Now let the second act alone: if the body were at A, it would move from A to D in a second; but the body is at B. Keeping, then, the direction and magnitude of this second velocity the same, it will carry the body from B to C in a second, where B C is parallel and equal to A D. The result, then, of the two velocities acting successively is that the body arrives at C; therefore, by this second law of motion, this will be the result if they act simultaneously for a second. Now, it follows, from a proposition of Euclid, that if C D be joined, A B C D is a parallelogram; and it is obvious that a single velocity in the direction A C, and of the proper magnitude, would have the same result and carry the body from A to C in a second—and velocities may be measured by the space they will carry a body in a second of time;—therefore we may regard A B and A D, not as the spaces over which the body would be carried by the velocities in their respective directions, but as graphic representations of the velocities themselves; and hence we arrive at the following theorem:—

"If two velocities, represented in magnitude and direction by two straight lines meeting at a point, be simultaneously communicated to a body, their effect is exactly equivalent to the effect of a single velocity represented by the diagonal passing through the point at which the two straight lines meet, of the parallelogram of which the two straight lines are adjacent sides."

But we have seen in the last chapter that forces are to be measured by the velocities they will communicate in a unit of time. And forces, like velocities, may be graphically represented by straight lines. For a force is completely known when we know its point of application, its direction, and its magnitude; and a finite straight line is completely known when we know the point from which it is drawn, the direction in which it is drawn, and its length; therefore a force may be represented by a straight line drawn from the point at which it acts, in the direction in which it acts, and to a length that will contain as many units of length as the force it represents contains units of force. Hence the above proposition is true also of forces, and therefore—

"If two *forces*, represented in magnitude and direction by two straight lines meeting at a point, act upon a body, they will together be equivalent to a single *force* represented by the diagonal passing through the point at which the two straight lines meet, of the parallelogram of which they are adjacent sides."

We are now in a position to point out the effect of this knowledge of the laws of motion upon men's views of physical astronomy. We have seen that Kepler and Descartes both thought that the planets, if left to themselves, would stop, and accordingly they each suggested a means by which they might be pushed on in their courses. But as soon as the first law of motion was known, it was seen that they would go on of themselves if once started, and that the question to be answered was not, why they did not stop, but why they did not move uniformly on in a straight line.

The second law of motion supplies us with the answer to this question. For a planet at A (Fig. 8) is moving in the direction AA′ of the tangent, or touching line, to its orbit at the point A; but when it has moved on to B, it is moving in the direction B B′ of the tangent to the orbit at B. Its motion has, therefore, changed from the direction A A′ when it was at A, to B B′ when it is at B; and

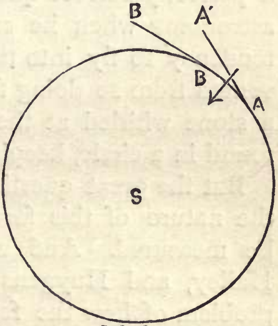

FIG. 8

this *change* of direction is in the direction of the arrow; therefore, since "change of motion . . . takes place in the direction of the straight line in which the force acts," it follows that some force must have acted on the body between A and B in the direction of the arrow, that is, towards the inside of its orbit. This is true of every point in its orbit, and hence there must be some force continually acting upon the planet—not a tangential force as Kepler and Descartes thought, but a "normal" force, that is, one which acts across the tangent, and continually bends it out of the natural straight course in which it would freely move, into the curved orbit we see it to have. Since the sun is inside the orbit of each planet, it was early suggested that the force in question acted towards his centre.

Borelli (born 1608, died December 31, 1679), a pupil of Galileo, very clearly saw this, and gave a perfectly correct qualitative account of physical astronomy when he said that each planet had a tendency to fly into the sun, but that it was prevented from so doing for just the same reason that a stone whirled at the end of a string will swing round in a circle, keeping the string tight.

But the great question to be answered was, what the nature of this force was, and how it was to be measured. And we find that Wren, Hooke, Halley, and Huygens all attempted to solve the problem of how the force must vary with the distance from the centre of the sun in order to cause the planets to move in ellipses; but they all failed.

The true law, however, was suggested by several persons about this time, apparently independently, though without logical reason; Bouillaud probably being the first, in 1645. The reason which seems to have had most weight with them was the analogy of light issuing from a luminous point. A flat surface placed at two feet from a luminous point will need to be four times as great as a flat surface placed parallel to it at one foot from the point in order to receive the same amount of light; at three feet it will need to be nine times as great, and so on. Hence the same finite flat surface will receive only one quarter the light at two feet off that it will receive at one foot off; at three feet it will receive only one-ninth, at four feet one-sixteenth, and so on. This is expressed by saying that the light received "varies inversely as the square of the distance." It was supposed that force issued from the centre of the sun in the form of rays, and that, in a similar manner, its intensity on a given planet would vary inversely as the square of the distance of the planet from the centre of the sun.

This was the condition of physical astronomy when the greatest man who appears in the history of science, and possibly the greatest intellect that has ever worked on earth, began to investigate the subject.

On the 25th of December (old style) in the year in which Galileo died, Isaac Newton was born in the manor-house of Woolsthorpe, a hamlet in the parish of Colsterworth, in the county of Lincoln. His father, Isaac Newton, died a few months after his marriage to Hannah Ayscough, of Market

Overton, in Rutlandshire, and before the birth of his son. The future astronomer was so weakly as an infant that his mother spoke of it afterwards as a marvel that he lived, and so diminutive that she declared "she could have put him into a quart mug." He seems, however, very early to have outgrown his physical weakness.

On the 27th of January, 1645, his mother married the Rev. Barnabas Smith, Rector of North Witham, and Newton was handed over to the care of his grandmother, Mrs. Ayscough, who henceforth lived at Woolsthorpe. Their property seems to have brought in about £85 a year, and this income was increased by the cultivation of the little farm surrounding the manor-house.

After learning to read and write at the day schools of Stoke and Skillington—two little hamlets about a mile to the north of Woolsthorpe—Newton was sent, when twelve years old, to the grammar school at Grantham. For some time he was extremely idle, and made no progress in his studies; but one day, having fought and beaten a bully much bigger than himself, he seems to have been stirred up by the incident to greater exertion in other directions, and soon rose to the top of the school. Still, like most great men of science, he was not remarkably clever as a boy, and the only indication of scientific ability was, as in the case of Galileo, his love of constructing working models of machines, in particular water-clocks. In order easily to set these, he observed the position of the sun by means of the shadows cast by pegs fixed into a wall, and in the course of a few years he managed so to correct the marks he made on the wall to denote the position of the shadow at the hours, as to make a sun-dial of considerable accuracy. This seems to have been his first introduction to astronomy.

In 1656 Newton's stepfather died, and his mother returned to Woolsthorpe, with her three children, Mary, Benjamin, and Hannah Smith. At this time he left school, and for some years lived at home, apparently with the idea of becoming a farmer, as his ancestors had been. He, however, paid little attention to his business, and his uncle, the Rev. W. Ayscough, who was Rector of Burton Coggles, about three miles east of Woolsthorpe, having found him one day studying some mathematical problem instead of looking after his farm, advised that he should be sent to college. Accordingly he was sent back to Grantham School to prepare for this; and on the 5th of June, 1661, was admitted a sub-sizar at Trinity College, Cambridge.

Before Newton left Woolsthorpe his uncle had given him a copy of Sanderson's "Logic," and when in the beginning of his college career he attended lectures on this subject, he found that he knew more of it than his teacher. This was recognized by his tutor, and he was admitted instead to lectures on Kepler's "Optics." This book was soon mastered by Newton at home, and he began to turn his attention to other subjects. He had bought a book on astrology at Stourbridge fair, but finding it impossible to understand it without a knowledge of geometry, of which even at this time he appears to have known but little, he purchased a Euclid; but the earlier propositions seemed to be so self-evident that he laid it aside "as a trifling book." He now took up Descartes's "Geometry," and this he seems to have found difficult; but after repeated attempts to overcome the various points that puzzled him, he finally mastered them all without the help of a tutor. The different ways in which he regarded these two works illustrate the bent of his mind afterwards shown in his own mathematical researches. He always preferred, and even seems to have found easier, the direct synthetic method of the old geometry to the algebraical or analytical method introduced by Descartes, and since developed into the most powerful instrument of scientific reasoning.

The mastery of Descartes's "Geometry" seems, however, to have brought out his great mathematical powers; for in the years 1664 and 1665 much of his work on infinite series and the quadrature of curves was done. So severely did he work at these subjects that an illness was brought on, which was further intensified by sitting up at night to watch a comet that appeared in 1664. From this illness we are told that "he learnt to go to bed betimes."

On the 28th of April in that year, he was elected scholar of Trinity College, Dr. Barrow being his examiner, and reporting that his knowledge of Euclid was but poor. In January, 1665, Newton took his degree, but no record has been left of the order in which the successful candidates appeared on the class list.

It is now that Newton's great scientific career began. In this year he for the first time committed to writing his ideas on the mathematical method of fluxions, which formed the germ of the differential calculus. And in this year also the first idea of the law of gravitation occurred to him. For, according to the account given by his niece, Catharine Barton, to Voltaire,* having been driven away from Cambridge by the Plague, he was sitting in the garden at Woolsthorpe, thinking of the laws of motion, when he saw an apple fall from a tree. The apple being at rest on the tree, it could only fall because some force acted upon it towards the centre of the earth. This force, which gives bodies their property of weight, was called "gravity," even at that time; and it seems to have struck Newton that this force of gravity acted upon the apple as well up in the tree as upon the ground, that it acts upon bodies on the summits of the highest hills as in the depths of the deepest valleys; and then the great thought struck him—Might not

* This story of the apple is sometimes doubted. It rests on the authority given in the text, and on the authority of Martin Folkes, President of the Royal Society. It was not mentioned in the two accounts given by Sir Isaac Newton himself to Pemberton and Whiston; but there is nothing in his account inconsistent with it.

this very same force extend outwards from the earth's surface as far as the moon, and be the cause which bends her path out of the natural straight course in which she would freely move, into her curved orbit surrounding the earth?

We have seen, that ever since the discovery of the laws of motion by Galileo, men had known that some force must act upon the moon at every point towards the interior of her orbit, and it had been suggested that it might act towards the centre of the earth; but no one had ever conceived that the familiar force of gravity, that gives the bodies we have to do with, their property of weight, was the very force that held the moon in her course.

No sooner did this idea occur to him, than Newton set to work to find the law in which it acted. The only one which seemed to apply at all naturally was the "law of the inverse square," as explained above, and so he sought whether his idea would be verified on this hypothesis.

We have seen that forces are to be measured by the velocity they will add on in a given time. It may be deduced mathematically from this that the space through which a body will be moved from rest under the action of different constant forces in a given time will be proportional to the velocities added on by the forces in the time; and hence the space through which a body will move from rest in a given time under the action of a force may be taken as a measure of the force.

Now, if the force of gravity varies inversely as the square of the distance from the centre of the earth, any distance we can move through vertically from the surface of the earth is so small that the variation of the force will be insensible, and therefore gravity may, without appreciable error, be taken as constant for all bodies we can handle. Now, bodies we can experiment upon near the earth's surface will fall through about sixteen feet in a second from rest, and Newton knew that the moon was about sixty times as far from the centre of the earth as the earth's surface is; hence, according to the law of the inverse square, the moon would fall from rest through $\frac{16}{60 \times 60}$ feet in a second, and from this it follows, since the space described varies as the square of the time, that the moon ought to fall through sixteen feet from rest in a minute. He now set to work to find how much the moon actually was pulled through in a minute from rest. In order to do this, it was necessary to know the distance of the moon from the earth, and as our knowledge of this depends upon the fact that the moon is about sixty radii of the earth distant from her centre, it will be well to explain how this was ascertained.

If a telescope be fixed to swing about a horizontal axis pointing due east and west, it will always be directed to some point in the sky which is on the celestial meridian, and, when horizontal, will point due north or due south; the plane in which it swings, therefore, passes through the centre of the earth, the north pole, the south pole, and of course the place where the telescope is. It is obvious from this that the great circle in which this plane cuts the earth's surface will be the terrestrial meridian of the place, and therefore the plane in which a telescope, mounted as just described, will swing, will be the same so long as the telescope lie on the same terrestrial meridian; and hence it follows that all places on the same terrestrial meridian have the same celestial meridian.

In Fig. 10, let the plane of the paper represent this plane of the meridian of two observatories, one of which will therefore be due south of the other. Let P A B E be this meridian, and C the centre and P the north pole of the earth; make P C E a right angle, then E will be the point where the meridian cuts the equator of the earth. It has been pointed out in the first chapter that the apparent height of the pole above the horizon at any place is equal to the latitude of the place. Observe the height of the pole at one observatory, and make the angle E C A equal to this angular height,—then A must be the point which represents this observatory; similarly we can find the point B, which must represent the second observatory; and then C A produced, or A Z, will be the direction of the zenith at A, and C B produced or B Z′, will be the direction of the zenith at B. Now, when the moon passes the celestial meridian of A or B, it must be represented by some point in the plane of the paper. In order to find that point, all we have to do is to

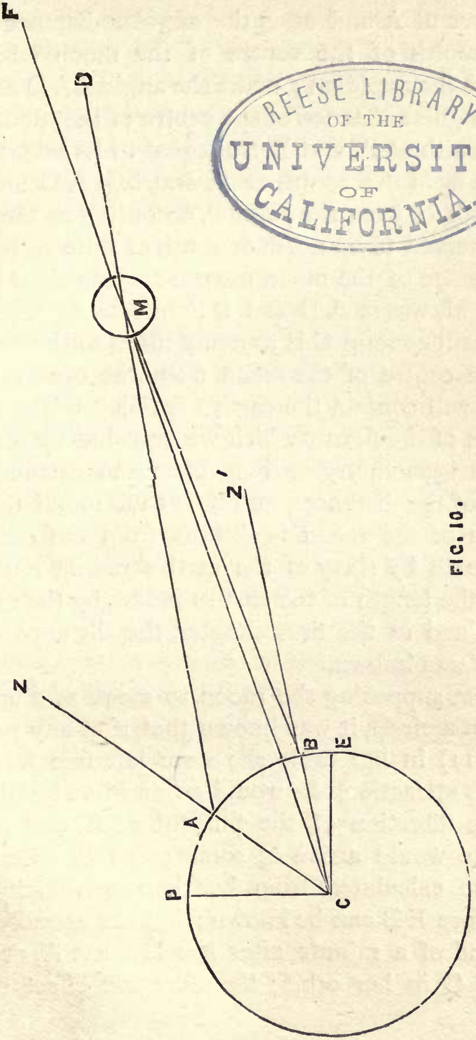

FIG. 10.

observe at A and at B the angular distances from the zenith of the centre of the moon when she passes the meridian ; make the angle Z A D equal to the angular distance of the centre of the moon from the zenith at A, and Z′ B F equal to its angular distance from the zenith at B, drawing A D and B F towards or from the point P, according as the moon is observed to be north or south of the zenith ; then the centre of the moon must be represented by the point M, where A D and B F intersect. Therefore C M will contain C E as many times as the distance of the centre of the moon from the centre of the earth will contain the earth's radius. Given all the angles of the figure, which we can find by observation, trigonometry will enable us to calculate the ratio of the distances, and it is thus found that the centre of the moon is distant from the centre of the earth by sixty of the earth's radii. Knowing, then, the length of the earth's radius, by the method explained in the first chapter, the distance of the moon was known.

Now, supposing the moon to move at a uniform rate in a circle, it was known that, if at any point A (Fig. 11) in her orbit she were left free from the earth's attraction, she would go on in a straight line in the direction of the tangent at A, and in one minute would arrive at some point B, where A B can be calculated from her known velocity, and therefore E B can be known ; but she actually is, at the end of a minute after she has left A, at some point C in her orbit ; therefore the effect of the earth's attraction is to drag the moon through a distance equal to the difference between E B and E C, both of which can be calculated. There are several sources of error in this argument, but when we take so small a time as a minute, they all become so small in proportion to the result, that it may be taken as very nearly true.

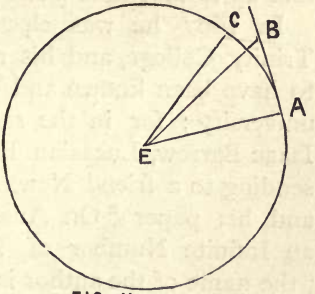

FIG. 11.

However, when Newton made the calculation, he found the moon was only pulled through thirteen feet in a minute, instead of the sixteen it ought to be if his theory were true. This error was much too big to be accounted for by the possible error of the method, and accordingly he "laid aside at that time any further thoughts of this matter."

He now devoted most of his attention to the subjects of optics and alchemy. In the latter he did not make much progress; but his discoveries in optics first brought him before the scientific world. As, however, they have no direct bearing upon the theory of astronomy, we have no space to consider them here. One application of his investigations must, however, be just noticed. This was the invention of the reflecting telescope—an instrument that could be made with less difficulty, and seemed, in the elementary condition in which the science of optics then was, to be capable of greater development than the old refractors which had up to that time alone been made.

In 1667 he was elected a Minor Fellow of Trinity College, and his mathematical skill seems to have been known and much thought of in the university; for in the summer of 1669 we find Isaac Barrow, Lucasian Professor of Mathematics, sending to a friend Newton's work on "Fluxions," and his paper "On Analysis by Equations with an Infinite Number of Terms," and stating that "the name of the author is Newton, a Fellow of our college, and a young man, who is only in his second year since he took the degree of Master of Arts, and who, with an unparalleled genius, has made very great progress in this branch of mathematics."

About this time Dr. Barrow resolved to devote himself wholly to theology, and resigned the Lucasian chair in favour of Newton, who was elected in his place on the 29th of October, in the twenty-sixth year of his age.

Next year he seems to have been first brought into contact with "some very eminent grandees of the Royal Society," and to have written notes on Kinkhuysen's "Algebra," at their request. This led to his election to their body on the 11th of January, 1672, and after this his most important discoveries were always communicated first to the society. The earliest of these communications related to optics, and they were attacked, with a virulence that showed that the spirit of the Aristotelians was not yet dead, by several foreign philosophers so called, among whom the chief were a Jesuit named Ignatius Pardies, and Francis Linus, a physician at Liége. The vexation and drudgery of having to carry on a discussion with men to whom it was necessary, not only to point out facts and to apply arguments, but even to explain the true method of scientific investigation, annoyed and mortified Newton; and in 1676 he writes to Oldenburg, the Secretary of the Royal Society, "I see I have made myself a slave to philosophy; but if I get free of Mr. Linus's business, I will resolutely bid adieu to it eternally, excepting what I do for my private satisfaction, or leave to come out after me; for I see a man must either resolve to put out nothing new or to become a slave to defend it."

His vexation and perplexities were about this time still further increased by pecuniary difficulties, and there is reason to think that he seriously contemplated giving up mathematical speculations, which he described, in 1674, as "at least dry, if not somewhat barren," and taking to the study of law.

In the beginning of 1673 Newton had written to Oldenburg, begging to resign membership of the Royal Society. The secretary, however, seems to have divined his reason for this step, and on the 28th of January, 1675, he represented that Newton was in such circumstances that he should be excused the weekly payments to the society. This was accordingly done, and we hear no more of his resignation.

As Newton was not in orders, his Fellowship would expire in the autumn of this year, but his pecuniary difficulties were overcome by obtaining, on the 27th of April, 1675, a patent from the Crown, permitting the Lucasian Professor to hold a Fellowship without the obligation to take orders.

In the same year his attention was again directed to astronomical subjects, though not to the theory of gravitation. We have seen that Galileo had discovered and explained the diurnal libration of the moon; Hevelius had found that she turned the face that fronts the earth sometimes a little to the east, sometimes a little to the west, in further periods that are not diurnal, and sometimes also a little to the north, sometimes to the south, in like periods, enabling an observer on the earth to look a little way round her edge in every direction at different times. Newton explained that the turnings to east and west, or the "libration in longitude," would be caused by the moon rotating at a uniform rate, while she does not move in her orbit, or *revolve*, at a uniform rate. As she completes a *rotation* in the same time that she completes a *revolution*, it follows that when she is revolving slowly her rotation is comparatively fast, and therefore, since she both rotates and revolves from west to east, she appears to an observer on the earth to turn her face a little to the west in the sky; and similarly, when she is revolving fast, she seems to turn her face a little to the east.

The turnings to north and south, or the "libration in latitude," he explained by referring it to the fact that the moon's axis of rotation is inclined to the ecliptic at an angle of 88° 17′. When, therefore, the moon is in such a position in her orbit that the northern part of her axis is directed towards the earth, the observer will see a little round her northern edge, that is, she will appear to turn her face a little to the south; and similarly, when the northern part of her axis is turned away from the earth, she will appear to turn her face a little to the north. These explanations Newton gave in a letter to Nicholas Mercator, who published them in his "Institutiones Astronomicæ," in 1676.

We have seen in the beginning of this chapter that the problem of finding what the law of attraction must be to cause a body to describe an ellipse with the centre of force in the focus, was one which was clearly proposed and considered before Newton published its solution. In the beginning of 1684, Halley, Wren, and Hooke were discussing it in London. They all seem to have thought the inverse square was the law, but, Halley says, none of them possessed a "convincing proof" of it. Hooke, however, maintained that he had one by him, but refused to produce it until the world, by trying, found out how difficult it was, and would therefore appreciate the ability of its discoverer. Hooke is one of the saddest figures in the history of science. As far as we can gather, he was a man of transcendent genius, which was entirely marred by vanity; and it is painful to contrast his vain-glorious boastings with the simplicity of the really great, humble man.

In August Halley set out for Cambridge, to consult Newton, who had no knowledge that these discussions were going on. Without mentioning anything of his deliberations with Wren and Hooke, he asked what path a body would describe under the action of a central force varying as the inverse square of the distance from the centre. Newton at once calmly replied, "An ellipse." Halley was greatly struck with the quiet confidence of this answer, and asked him how he knew this. Newton replied that the problem had occurred to him about fifteen or twenty years before, and he had solved it. The greatest minds in the world were puzzling over this complex problem, and to Newton it had occurred, and he had solved it, and never knew that he had done a great thing. It came quite naturally to him, and when asked to produce his paper on the subject, so little had he thought of it that he could not find it.

Halley, however, seems to have convinced him of the importance of this work, and accordingly he promised to forward to the Royal Society a treatise, "De Motu," which should contain this and other demonstrations of great interest.

Another event tended to concentrate his attention on the subject of gravitation. About this time he seems to have heard that a new measure of the earth had been made by a French astronomer, Picard, according to which the distance of the moon was seen to be greater than he had thought it in 1665; and therefore it was possible that his old idea of the moon being held in her orbit by the same force that gives bodies their property of weight near the earth's surface, might after all be true. Newton turned to his old calculations with intense earnestness. All that was necessary was to substitute the new value of the moon's distance for the old one, and work out the simple arithmetical sum explained above—not a hard task for the greatest mathematician the world has ever seen; but as he was nearing the end of it, he saw that it was coming right, and so great was his excitement that he could not finish the sum.* A friend completed it for him; and the key to the mystery, the clue to the great secret, was found at last.

* This story is mentioned by Dr. Robison, but he does not give his authority for it.

CHAPTER VII.

ON NEWTON'S "PRINCIPIA."

NEWTON now awoke to the full grandeur of his mission, and for the next two years the whole strength of his stupendous intellect was devoted to the complete unravelling of the intricacies of physical astronomy.

It will be well to give here some account of the mighty work in which these studies were given to the world. The "Philosophiæ Naturalis Principia Mathematica," of which Laplace has said that "the number and generality of his discoveries, . . . the multitude of original and profound views which have been the germ of the most brilliant theories of the geometers of last century, . . . will assure to the 'Principia' a pre-eminence above all the other productions of the human intellect." *

The work consists of an introduction and three books. The introduction is divided into two parts,

* "Exposition du Système du Monde," p. 336.

the first of which is concerned with "definitions" of the terms which he uses throughout the work; the second consists of a discussion of the "axioms or laws of motion" which lie at the basis of all physical science; and it is as great, though perhaps scarcely as striking, a proof of Newton's genius as any he has left us, that the whole progress of scientific thought for the last two centuries has failed to improve upon the statement of the foundations of science there given by him, and given for the first time, to the world. The laws are three in number. Two of them we have given in his words in the last chapter; the third we may state here.

"To every action there is always an equal and contrary reaction; or, the mutual actions of any two bodies are always equal and oppositely directed."

The first book treats of the motion of bodies in free space; the second, of the motion of bodies in a resisting medium, and of oscillating motion, as of a pendulum; the third, of the system of the world.

The first book is divided into fourteen sections, and contains ninety-eight propositions, besides a number of corollaries, lemmas, and scholia. In the first section the author explains the method of geometry which he uses throughout his work, and which is essentially the same as that of the differential and integral calculus. In the second section he considers central forces, and the laws they must obey to produce motion in various given orbits.

The third section has to do with the law of force that will produce motion in a conic section. The fourth and fifth are only concerned with certain questions relating to the geometry of the conic sections; the sixth, with the motion of a body in a given orbit; the seventh, with the motion of a body in a straight line, under the action of a central force in that line; the eighth, with the orbit described under the action of a given force; the ninth, with motion in movable orbits; the tenth, with motion over given surfaces. The eleventh deals with the motion of bodies mutually attracting one another; the twelfth, with the attractions of spheres; the thirteenth, with the attractions of bodies of other form; and the fourteenth with certain attractions of interest in Newton's physical theory of light.

The second book consists of nine sections, and contains fifty-three propositions, with none of which, however, we shall have to do. The second lemma to the eighth proposition contains the method of fluxions which was Newton's great mathematical creation.

The third book commences with the "rules of reasoning in philosophy," and a statement of the "phenomena, or appearances," of the solar system, which are merely Kepler's laws. The rest of the book consists of forty-two propositions. The first eighteen are concerned with the attractions of the sun, moon, and planets; the nineteenth and twentieth, with the figure of the earth; the twenty-first, with the precession of the equinoxes; the next two, with the irregularities in the motions of the moon and of the satellites of Jupiter and Saturn; the twenty-fourth, with the tides. The quantitative computation of the phenomena he has explained qualitatively in the last four propositions occupy the next fifteen. The remaining three deal with the theory of comets. And the whole wonderful work closes with an outburst of awe and admiration of the Eternal and Infinite Being who, as he there says, "rules all things, not as a mere spirit of nature, but as Lord of all."

The great result at which it arrives is called the law of gravitation, and is unquestionably the most remarkable discovery ever made by the mind of man. It may be stated as follows:—

"Every particle of matter in the universe attracts every other particle with a force varying directly as the product of their masses and inversely as the square of the distance between them."

For a complete proof and verification of this law, even the mathematical methods of Newton are inadequate; and only a very rough sketch of the reasons for which we believe it to be true can be given here. For this reason it will, perhaps, be clearer not to follow Newton's order.

There are six distinct steps in the investigation of the law of gravitation, and they are the following:—

1. It is one and the same force obeying this law which gives bodies their property of weight on the

earth's surface and that holds the moon in her orbit.

2. The force that holds a planet or satellite in its orbit is directed accurately towards the centre of its primary.

3. It is a force obeying this law with which the sun holds each individual planet in its elliptical orbit.

4. It is this same force with which the sun holds the different planets in their different orbits.

5. This same force is exerted between bodies of the solar system which do not revolve round one another, and hence irregularities, or "perturbations," are introduced into the form of their orbits.

6. These forces do not take place by centres of attraction residing at the centre of each body of the solar system, but they are due to the fact that *every particle* of matter in the system attracts every other particle with a force acting in accordance with this law.

Before we examine these steps in order, it will be well to explain what is meant by the "mass" of a body or particle, or the "quantity of matter" in it, for thereby the argument will be simplified.

Consider two bodies of masses M_1 and M_2 respectively, separately attracted by a third body of mass M_3 at the same distance from it; then, by the law of gravitation, the distances being the same in both cases, the forces on M_1 and M_2 will be directly proportional respectively to the product of M_1 and M_3, and to the product of M_2 and M_3; but M_3 is the same in both cases, therefore the forces will be respectively directly proportional to M_1 and M_2. Hence the masses of a number of bodies are proportional to, and therefore may be measured by, the fôrces separately exerted on them by some one other body at the same distance from each. In the case of bodies on the earth's surface, the earth itself may be conveniently taken as this other body, and then we see that the masses of bodies, or the quantities of matter in them, are proportional to, and may therefore be measured by, their weights.

One other important fact may be explained at this point. The forces separately exerted upon two different bodies by a third body are, by the second law of motion, proportional to the change of motion produced in them—change of motion, as was explained in the last chapter, being proportional to the mass of the moving body, and the change in its velocity conjointly. But the forces separately exerted on the two bodies by a third body are, we have seen, respectively proportional to the masses of the two bodies; therefore, if there be any variation in the forces on the two bodies other than in the proportion of their masses, it will be measured by the change of *velocity* produced in them; and hence, in particular, the law of gravitation between the centres of the bodies of the solar system will be proved if we can show that the changes of velocity in any two bodies due to the attraction of a third are inversely proportional to the square of their distances from it.

But let us turn now to the consideration of the six steps in the investigation of the law of gravitation.

1. The first step we have already considered. To make it perfectly clear, however, we must point out that when the time is fixed and taken so short that the space described in it does not sensibly alter the distance from the attracting body, the changes in space described may be proved mathematically to be proportional to the changes in the velocity produced in the time in the two bodies, and therefore may be taken to measure the forces. On this principle we have seen that Newton found that, if the force between the earth and the moon on the one hand, and the earth and any other body on the other, be taken as directly proportional respectively to the products of the masses of the earth and moon on the one hand, and of the earth and the body on the other, then the forces will be inversely proportional respectively to the squares of the distances between the earth and moon on the one hand, and the earth and the body on the other.

2. In the above we have assumed that the forces in question are mutual attractions between the *centre* of the earth and the *centre* of the moon. Newton proved from Kepler's second law that this is the case with the sun attracting the planets; that is, if one body move about another so that the line joining their centres sweeps out equal areas in equal times about the centre of the second body, the first body must be acted on by a force

directed from its centre to the centre of the second body.

To prove this, we must assume a proposition proved by Euclid, that if two triangles stand upon the same or equal bases, and are equal in area, they are of the same height from that base or those bases.

Now, suppose a body to move so that some point in it describes a polygon A B C D E (Fig. 12); and

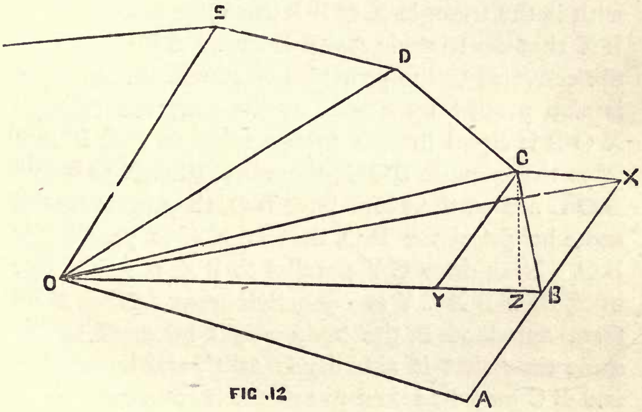

FIG. 12

suppose that point describe each side of the polygon in the same time—say a second—and at a uniform rate, different for each; and suppose there be some point O such that the triangles A O B, B O C, etc., are all equal in area; then the point will sweep out equal areas round O in successive seconds: and by the first law of motion no force acts upon the point while it is describing each side of the polygon ; but a force must act for an instant at each angular point, to deflect it out of its natural straight path.

Consider the body when it is at B. If no force acted on it when it was at B, it would during the next second move on to X, where A B X is a straight line and B X equals A B. Join X O and X C. Then the triangle X O B stands upon the base X B, which is equal to A B ; and, being in the same line with it, the triangle X O B is the same height above B X that the triangle A O B is above A B ; hence, by the converse to the proposition stated above, which is also proved by Euclid to be true, the triangle X O B is equal in area to the triangle A O B, that is, to the triangle B O C ; therefore, since X O B and B O C are on the same base B O, they must be the same height above B O, that is, X C is parallel to B O. Now draw C Y parallel to B X, meeting B O in Y, then B X C Y is a parallelogram. Now, if no force acted at B, the body would be at X at the same time that it actually is at C, and hence B X and B C may be taken to represent respectively the original velocity of the body and the whole velocity of the body during the second second of its motion; therefore, by the parallelogram of velocities, since B X C Y is a parallelogram, B Y represents the velocity given to the body by the force at B, which must be in the nature of a blow ; therefore the blow at B acts towards O. Similarly, the blows at

C, D, E, and all the angular points of the polygon, act towards O.

Now, suppose the polygon to consist of a very large number of extremely small sides, and the body to describe each of these in one-millionth of a second, then in one second it would have described a million of these little straight paths, and have received a million taps all towards O. Hence it has *very nearly* described a curved path under the action of a continuous force towards O. The argument still holds good if we diminish the lengths of the sides of the polygon without limit, and increase without limit their number; hence it follows that, if a particle describe a path in such a way as to sweep out equal areas in equal times about a point in its plane, it must be acted on by a force towards that point. Therefore it follows, from Kepler's second law, that the forces retaining the planets in their orbits act at their centres towards the centre of the sun.

The same also is true of the force retaining the satellites in their orbits round their primaries; for Kepler's laws were introduced into the theory of Jupiter's satellites by himself, and into the theory of the moon by Horrox, in an essay sent to Crabtree in 1638, although her great inequalities—the evection, variation, and annual equation—still would not yield to the explanation; to account for these, Horrox still retained a system of epicycles. Kepler's laws have been found to hold good for all the satellites and planets discovered since that time.

3. We now have to consider how the force on each planet must vary in order to hold it in its elliptic orbit. It will be well here to explain what an ellipse is. If a plane surface cut a cone, the line on the surface of the cone along which the plane cuts it is some curve which is called a conic section. If the plane cut the cone completely across in a slanting direction, the curve is of an oval form, and is called an ellipse. It may be deduced from this definition that there are two points inside the ellipse such that the sum of the distances of any point on the curve from these two fixed points is the same at whatever part of it the point on the curve be taken. Hence, in order to draw an ellipse on paper, fix two pins into the paper, tie a continuous string loosely round the pins, and stretch it tight with the point of a pencil on the paper; the line which can be traced out by the pencil, always keeping the string tight, will be an ellipse, and the two pins are called the foci of the ellipse. In Fig. 13, S and S′ are the foci; the straight line A A′, drawn through the foci and terminated by the curve, is called the major axis; the point C in the major axis, half-way between S and S′, is called the centre of the ellipse; the line B B′, through C at right angles to A A′, is called the minor axis; and the line L L′, through either focus at right angles to A A′ and terminated by the curve, is called the latus rectum.

Now, suppose Kepler's first two laws to hold good, as they do, for the bodies of the solar system; suppose, that is, a planet to move in an ellipse sweeping out equal areas in equal times about one focus. Take three positions of it at A, C, and E

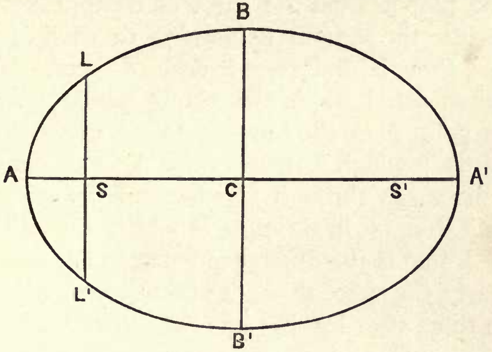

FIG. 13

(Fig. 14), such that S A is double of S C and triple

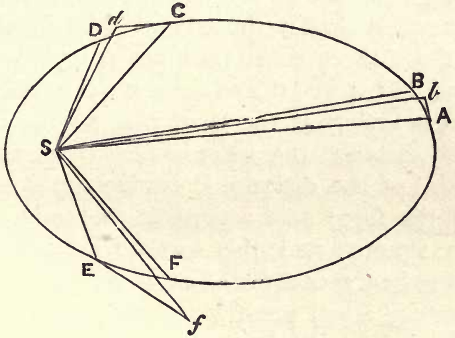

FIG. 14.

of S E, and set off three very small triangles, A S B, C S D, and E S F, in the ellipse, all equal in area.*

* We have to exaggerate their size in the figure, in order to see the lines with which the argument is concerned.

Then the planet will describe A B, C D, and E F in equal times; and, if these are all small, the distance of the planet from the sun will be approximately the same throughout its motion in each of these spaces, though different for each of them. Now, if no force acted at A, the planet would go on to some point *b* in the tangent to the orbit at A, in the time in which it moves from A to B, and therefore the space through which the force of the sun upon it drags it in the time in which it would move from A to *b* is the difference between S *b* and S B; similarly, the space through which it is pulled in the same time after leaving *c* is the difference between S *c* and S C, and the space through which it is pulled in the same time after leaving E is the difference between S *f* and S F. Now, it is capable of calculation that if the triangles be indefinitely small, these three differences are respectively proportional to 1, 4, and 9, and these are proportional to the forces acting at the three places; therefore at half the distance the force is four times as great, at a third of the distance it is nine times as great, that is, the force acts according to the law of the inverse square; and this can be mathematically proved to be the case for all other relative distances. The mathematical proof we, of course, cannot give in this brief sketch, but we may indicate its nature.

Suppose the whole area of the ellipse to be divided into infinitesimally small triangles, each of which may then be supposed to have that side of it which lies in the curve, as well as the other two, straight. We must exaggerate the figure in order to follow out the proof. Let B O C (Fig. 12, p. 173) be one of these triangles: it follows, from the proposition of Euclid used in proving the second step, that if C Z be drawn perpendicular to O B, the area of the triangle B O C is proportional to the product of B O and C Z. Now, each of these triangles will be swept through by the line joining the centre of the planet to the centre of the sun, or the "radius vector" of the planet, in the same time; and in sweeping out B O C, the planet is, by the proof of the second step, pulled through B Y by the force exerted by the sun acting while the planet moves from B to C, which, when B O C is infinitesimally small, is the same thing as the force exerted by the sun on the planet at B. But the space through which it would have been pulled in any other time would, as Galileo found, be to B Y as the square of the other time is to the square of the time which it takes to describe B C; that is, as the square of the area swept out in the other time is to the square of the area B O C. Therefore the space it is pulled through in unit time is to B Y as the square of the area swept out in unit time is to $(BOC)^2$; that is, the space pulled through in unit time with the force at B is proportional to $\frac{BY}{(BOC)^2}$, that is, to $\frac{BY}{(BO \times CZ)^2}$, or $\frac{BY}{BO^2 \times CZ^2}$. But the force at B is proportional to the space through which it would pull the planet in a unit of time, therefore the force at B is proportional to $\frac{BY}{BO^2 \times CZ^2}$. Now, it is shown by Newton, as a property of the ellipse, that $\frac{BY}{CZ^2}$ is constant for all the equal triangles similar to B O C when they are taken infinitely small, and under these circumstances that $\frac{CZ^2}{BY}$ is equal to the latus rectum; hence the force at B is proportional to $\frac{1}{BO^2}$ or is inversely as the square of the distance from the sun at O.

4. So far we have seen that the law of gravitation holds between the sun and each planet moving separately in its elliptic orbit considered as particles of matter condensed at their centres. We must next show that it holds for the sun attracting the different planets moving in their different orbits but still considered as particles of matter. It might have been that the force on Mars at M_1 (Fig. 15), was to the force on Mars at M_2 in the ratio described by the law of gravitation, and the force on Jupiter at J_1 to that on Jupiter at J_2 in the ratio described by the same law, and yet the force on Mars at M_1 might *not* be to that on Jupiter at J_1 in the ratio described by the law. It is our object to show that this is the case. Here again, however, we can only indicate the nature of the proof. It depends upon Kepler's third law.

We have already shown that, if the law of gravitation be true, the spaces through which the different

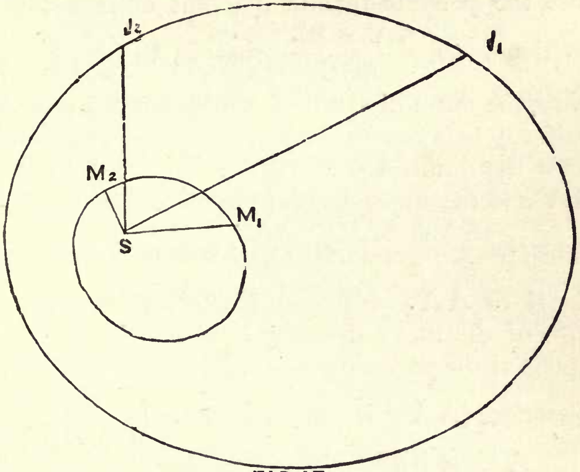

FIG 15.

planets are pulled in a given infinitesimally small time will be proportional to the forces exerted upon them by the sun in those times; hence all we have now to prove is that these spaces are inversely as the squares of the distances.

Now, the area described is proportional to the time of description in each ellipse; therefore the periodic time is to the unit of time as the whole area of the ellipse is to the area described in the unit of time. Let us take the unit of time indefinitely short, then as before B O C (Fig. 12, 173) may be taken to represent the area described in the unit of time, and it may be proved that the area of any ellipse is proportional (when we are comparing different ellipses) to the product of the major and minor axes; therefore the periodic time in different ellipses is proportional to $\frac{AA' \times BB'}{BOC}$, that is, to $\frac{AA' \times BB'}{CZ \times OB}$. Now, the mean distance of a planet is the distance half-way between its longest and shortest distance from the sun, and (Fig. 13) the longest distance is SA' and the shortest SA; therefore the mean distance is $\frac{SA + SA'}{2}$, or $\frac{AA'}{2}$; that is, it is proportional to AA'. Now, by Kepler's third law, the cube of the mean distance is proportional to the square of the periodic time in different elliptic orbits. Therefore $(AA')^3$ is proportional to $\left(\frac{AA' \times BB'}{CZ \times OB}\right)^2$, or to $\frac{AA'^2 \times BB'^2}{CZ^2 \times OB^2}$; therefore AA' is proportional to $\frac{BB'^2}{CZ^2 \times OB^2}$, or $\frac{BB'^2}{AA'}$ is proportional to $CZ^2 \times OB^2$. Now, it is proved to be a property of the ellipse that the latus rectum is to the minor axis as the minor axis is to the major axis, that is, the latus rectum equals $\frac{BB'^2}{AA'}$; but the latus rectum equals $\frac{CZ^2}{BY}$, as we have stated above; therefore we find that $\frac{CZ^2}{BY}$ is proportional to $CZ^2 \times OB^2$, that is, BY is proportional to $\frac{1}{OB^2}$, or inversely proportional to the square of O B. Therefore, in different elliptic orbits, the space through which a planet is pulled in a unit of time by the sun varies inversely as the square of the planet's distance from the sun. Therefore the law of gravitation is true of the sun attracting the different planets in their different elliptic orbits.

We have in the above proofs assumed scarcely any mathematical knowledge, but the reader who understands the elements of algebra will be able to see that a complete proof may be very easily given of this if we assume the planets to move in circles whose radii are equal to the mean distances.

Let p be the periodic time of the planet, t some indefinitely short time in which the planet moves from A to C (Fig. 11), let r be its distance from the sun; the circumference of the orbit equals kr where k is some constant. Then the planet moves through kr in time p; therefore its velocity is $\frac{kr}{p}$; therefore in the time t it will move a distance $\frac{kr}{p}t$. Now, if no force acted on it after it left A, it would move to B, where A B equals $\frac{kr}{p}t$, and therefore E B, by Euclid, i. 47, equals $\sqrt{\frac{k^2r^2t^2}{p^2}+r^2}$, or $r\sqrt{1+\frac{k^2t^2}{p^2}}$; and therefore the space pulled through in the time t equals E B − E C, that is,

$r\sqrt{1 + \frac{k^2t^2}{p^2}} - r$. Expanding by the binomial theorem, this is $r + \frac{1}{2}\frac{k^2t^2}{p^2}r +$ terms involving the fourth and higher powers of $t - r$, that is, $\frac{1}{2}\frac{k^2t^2}{p^2}r +$ higher powers of t; these higher powers may be neglected when t is taken infinitely small, and then we get the space pulled through proportional to $\frac{r}{p^2}$, when the time remains the same, and this is, in different orbits, proportional to the force. But, by Kepler's third law, p^2 is proportional to r^3; therefore the force is proportional to $\frac{r}{r^3}$, or $\frac{1}{r^2}$, which proves the proposition.

This proof is only important to us here as having been probably discovered by Hooke and Huygens before Newton investigated the question. The only part of the law of gravitation which was found independently of Newton, then, was an incomplete portion of one of its six steps.

5. We have now to show that, not only does the law of gravitation hold good for the sun and each primary planet attracting each body that revolves round it separately, but that all the bodies of the solar system attract one another according to this law; still, however, considering them all as particles, or points of matter situated at their centres. But in this step, which was Newton's great triumph, it is essential to show that the magnitudes of the

effects produced are just those which the law of the inverse square, and no other, would produce. To prove this completely, even Newton's mathematical methods are inadequate; it will therefore be readily understood that we cannot give the proof with any completeness here. We may, however, show that forces following this law would produce the kind of irregularities from the motion described in Kepler's laws, which are actually observed to take place.

The only irregularities which had been observed up to the time we have arrived at in the history of astronomy were, as we have explained, the evection, the variation, and the annual equation of the moon. In the case of the planets, for reasons which we shall explain hereafter, these irregularities, or "perturbations," as they are called, are so small as to require very accurate instruments and methods of observation to detect them directly at all. Newton was, therefore, only concerned with the lunar theory in verifying the law of gravitation.

Now, it is important for the following investigation that we should know the distances and magnitudes of the solar system. We have seen how men found the distance of the moon; then, knowing her apparent angular diameter, we can see they might estimate her actual size in order to appear so large at such a distance. But we have not yet seen how the magnitudes and distances of the bodies of the solar system could be ascertained. We saw in the second chapter how the relative distances of the earth and other planets from the sun could roughly be found, and therefore we may draw a series of circles to represent about the relative magnitudes of the orbits of the planets. In Fig. 16 let the inner and outer circles represent respectively the orbits of the earth and the planet Mars, found in this manner. Now, observe at any

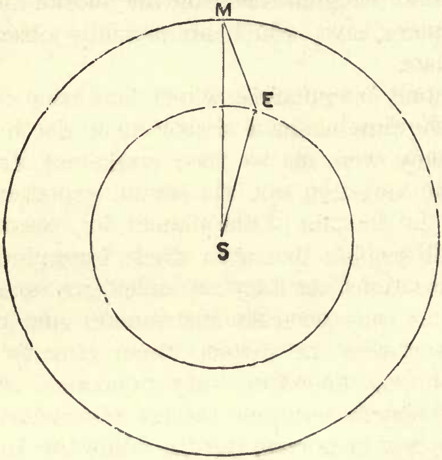

FIG. 16.

instant the apparent angular distance of Mars from the sun, and make the angle S E M equal to this angle, E being on the inner circle, M on the outer, and S at the centre. We have seen in Chapter III.* that the parallax of any planet may be observed, the diameter of the earth being the base. It follows, then, that, knowing the diameter of the

* *Vide* p. 77.

earth, we may, by the same method as that employed in finding the distance of the moon, find the distance of Mars when in the position M. Then the distance of the earth from the sun or of Mars from the sun is to this distance of Mars from the earth as S E or S M is to M E.

This method only gives a very rough result, the more accurate method of the transit of Venus we shall consider in next chapter. But by this rough method it could be ascertained that the sun was enormously further from the earth than the moon, and that no planet comes nearer to the earth and moon than one quarter the distance of the earth from the sun. Now, the planets look like mere points of light, but the sun looks as large as the moon; therefore the sun, never being less than four times the distance of the nearest planet, must be enormously greater in size than the planets, and therefore it is probable that his weight or his mass, which is proportional to his weight, is enormously greater than that of the planets; and hence, if the law of gravitation be true, his influence in perturbing the moon must be enormously greater than that of the planets. The question, therefore, for us to solve is—Will the mutual attractions of the sun, moon, and earth, according to the law of gravitation, account for the actual motion of the moon round the earth?

First. If the law be true, and the sun be enormously greater than the earth, will it not move round the sun and only be perturbed by the earth, rather than move round the earth and be perturbed by the sun? To this we may answer that it actually does move round the sun in a figure which is very nearly an ellipse, for the sun is four hundred times as far from the earth as the moon is, and therefore the earth carries the moon round the sun almost in her own orbit. And the moon's orbit, since she only describes thirteen revolutions round the earth while the earth describes one round the sun, is always concave to the sun.

Secondly. The sun being four hundred times as far from the earth, and therefore, on the average, from the moon, as the earth is, his great mass must be divided by 400×400 before we compare it with the small mass of the earth in order to find the relative attractions of the sun and earth on the moon.

Therefore we may consider that the moon moves round the earth according to Kepler's laws, but has irregularities introduced into her motion by the perturbing force of the sun.

Now, the plane of the moon's orbit is nearly the same as that of the ecliptic; we shall not introduce a material error into the investigation if we suppose them, for the present, to be quite the same.

Now, let S (Fig. 17) represent the sun, E the earth, and $M_1 M_2 M_3 M_4$ the orbit of the moon. The figure is necessarily exaggerated, and we must remember that E S is about four hundred times the radius of the circle. It is important to remember that the perturbing force of the sun is *not* the

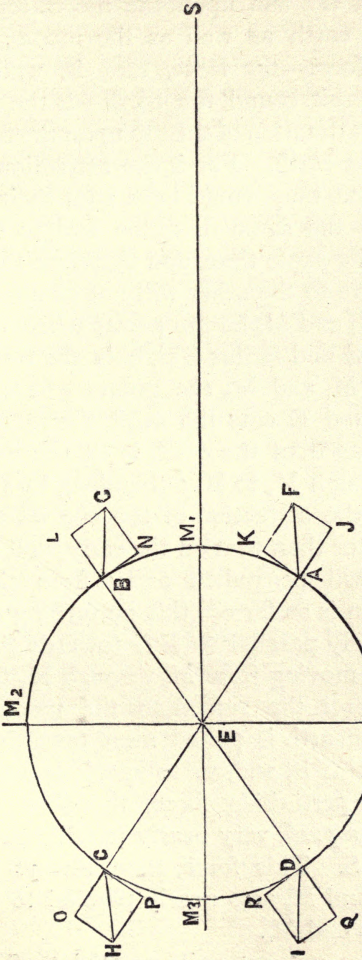

FIG. 17

attraction of the sun upon the moon, for the sun attracts the earth as well as the moon; and the perturbing force—the force, that is, which alters the moon's orbit round the earth—is the *difference* between the attraction of the sun upon the moon, and that upon the earth. For if the attraction on both were the same, they would both alter their position in space in the same direction and to the same amount in the same time, and therefore the motion of one relative to the other will not be altered.

Now, if M_2 and M_4 be the points where the circle, with centre S and radius S E, cuts the orbit of the moon, and M_1 and M_3 the points where the line through S and E cuts the orbit, the moon will be nearer the sun than the earth is all the way round from M_4 through M_1 to M_2; therefore, by the law of gravitation, the attraction of the sun on the moon will be greater than that on the earth, and therefore the moon will be pulled away from the earth. Since the sun is so far off, this perturbing force will act very nearly parallel to E S towards S. When the moon is moving from M_2 through M_3 to M_4, the earth is nearer the sun than the moon is, and therefore the earth is pulled more towards the sun than the moon is, and, so to speak, leaves her behind. The perturbing force, therefore, must be regarded as a *push*, very nearly parallel to S E, and away from S. This force, then, always tends to separate E and M, acts nearly parallel to S E, and is zero at M_2 and M_4.

But it does not act *quite* parallel to S E. The sun is always dragging the earth and moon along lines which converge at S, and therefore always tends to bring them together. This perturbing force, which may be considered as acting towards E, is strongest at M_2 and M_4, but, since S is so far off, is very small, but the force tending to separate M and E is zero at M_1 and M_3; therefore there are four points in the orbit at which the force tending to alter the distance of the earth and moon is zero; and these will lie two near M_2, one on each side, and two near M_4, one on each side, since the outward perturbing force is generally greater than the inward.

It is the outward perturbing force, however, that we have most to do with. It is obvious that, since we may combine two forces into one by the parallelogram of forces, we may also split up one into two by the same method. For if we have a single force represented by a line A C (Fig. 9, p. 145), and construct a parallelogram A B C D, with A C for diagonal, the force represented by A C must be exactly equivalent to two forces represented by A B and A D respectively. When the parallelogram is so constructed that A B and A D are at right angles, A B and A D represent forces which are called the "resolved parts," in their respective directions, of the force represented by A C.

Now consider the perturbing force on M when it is at some point A (Fig 17, p. 189) between M_4 and M_1. The perturbing force may be represented by A F. Join E A and produce it to J, draw F J perpendicular to E A produced, and complete the parallelogram A J F K; then the perturbing force A J will be exactly equivalent to two forces—A J along the radius vector of the moon outwards, and A K perpendicular to it and acting in the direction in which the moon is moving. Therefore between M_4 and M_1 the perturbing force acts outwards, and we have already seen it does so, all the way round the orbit, except just in the neighbourhood of M_2 and M_4, *and also* tends to quicken the rate of motion of the moon. By an exactly similar argument, we see that at any point B between M_1 and M_2 the perturbing force tends to retard the rate of motion of the moon, having a resolved part B M perpendicular to the radius vector, and in the opposite direction to that in which the moon is moving. From M_2, all the way round through M_3 to M_4, the earth is pulled more than the moon, and therefore the perturbing force must be considered as a push; and, by exactly the same method as before, we see that the force has a resolved part C P in the direction in which the moon is moving; therefore the moon's is accelerated, and similarly between M_3 and M_4 it is retarded.

So far we have not considered at all the fact that the moon's orbit is nearly an ellipse and not a circle. Let us now take this into account.

The two ends of the ellipse, A and A′ (Fig. 13), are called "apses," and A A′ is called the "line of apses." When the moon is at A (if the earth be in the focus S) it is in perigee, when at A′ in apogee.

It is easily seen from the figure that S A and S A′ are the only two lines which can be drawn from S_1 to be perpendicular to the curve.

Suppose now the perigee be exactly between E and S (Fig. 17), that is, at M_1. Consider the action of that part of the disturbing force which is resolved along the radius vector. When the moon is approaching perigee A (Fig. 18) and near it, suppose at M_1

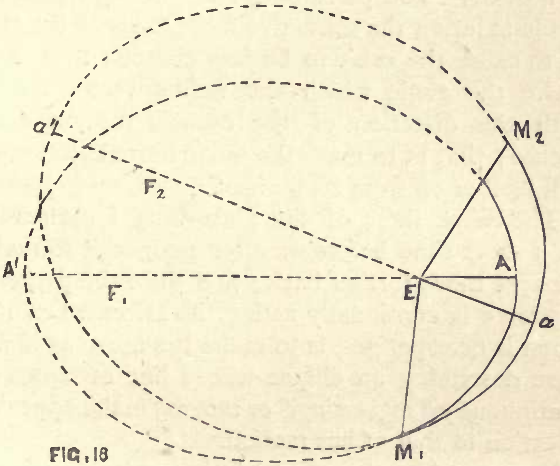

FIG. 18

the disturbing force acts outwards, and therefore lessens the attractive force towards E; therefore the orbit will be bent round less, and will lie outside the elliptic orbit in some position M_1 *a*, and then the perpendicular upon the new orbit will, in virtue of the fact that it is outside the old orbit, meet it at some point *a* nearer to M_1 than A is.

Suppose now the perturbing force to cease, the moon will then continue to move in an elliptic orbit whose perigee is at *a*.

Now let the perturbing force begin to act again when the moon has got a little past its new perigee at *a* to some point M_2. By the properties of the ellipse, it follows that the radius vector makes an obtuse angle with the direction in which the moon is moving. The perturbing force, acting outwards, tends to lessen the attractive force towards E; that is, to cause the orbit to be less curved; that is, to make the angle which the radius vector makes with the direction of the moon's motion *more* obtuse; that is, to make the moon move as if it were still further off from its perigee.

The effect, then, of the perturbing force acting for a short time before or after perigee is to make the apse move further back; and since the perturbing force is continually acting, its effect, when the moon is near perigee, is to make her move as if she were describing an ellipse whose line of apses is continuously "regressing," or moving in the opposite direction to that of her motion.

Now consider the effect of the perturbing force along the radius vector when the moon is near apogee at A′ (Fig. 19). Suppose the moon to be at M'_1, approaching A′. The radius vector makes an obtuse angle with the direction of her motion. When this angle becomes a right angle, the apogee will be reached. But the effect of the perturbing force, acting as it does outwards, is to diminish the attraction towards the earth; that is, to cause the new orbit to lie outside the old one; that is, to make the angle the radius vector makes with the direction of the moon's motion *more* obtuse; that is, to put further off the point where this angle is a right angle to some position a'; that is, the apse progresses. Similarly, when the moon has passed the new apse a', and is at M'_2, the angle between

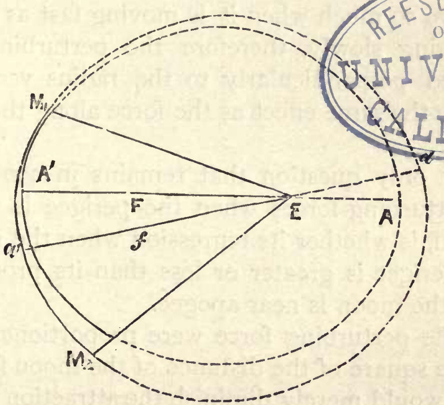

FIG. 19.

the radius vector and the direction of the moon's motion is an acute angle; the effect of the perturbing force is to make it less acute, and therefore to bring the point at which it is a right angle nearer to M'_2, that is, again to make the apse progress.

The general effect, therefore, of the perturbing force along the radius vector is to make the apse regress when the moon is near perigee, and progress when it is near apogee. The question now arises—What is the effect of the perturbing force resolved perpendicularly to the radius vector? We have seen that it is to increase the velocity of the moon when at perigee or apogee, when the line of apses passes through the sun. Now, a given force will not be able to cause the path of a body to curve so much when it is moving fast as when it is moving slowly, therefore the perturbing force resolved perpendicularly to the radius vector has exactly the same effect as the force along the radius vector.

The only question that remains in considering the perturbing forces when the perigee is towards the sun, is whether its regression when the moon is near perigee is greater or less than its progression when the moon is near apogee.

If the perturbing force were proportional to the inverse square of the distance of the moon from the sun, it would merely diminish the attraction towards the earth, but would leave it obeying the law of the inverse square; therefore the orbit would be a fixed ellipse, or the regression near perigee would be exactly equal to the progression near apogee. But this is not the case. It can be proved mathematically that, since the attraction of the sun on the earth and moon obeys the law of the inverse square, the *perturbing* force is nearly proportional at any moment to the moon's distance from the earth at that moment. And further, that the points at which the regression near perigee is changed into progression near apogee are at the extremities of the latus rectum passing through the earth. It follows from this that the perturbing force is actually greater near apogee than near perigee, and therefore much greater than is needed to balance the effect of the force at perigee; and further, since the moon moves nearly in accordance with Kepler's second law, it has a longer time to act; therefore, on the whole, the progression of the perigee will outweigh its regression. All this argument exactly applies when the perigee is turned directly away from the sun; therefore, when the line of apses passes through the sun, the perigee on the whole progresses.

Now consider what happens when the earth has moved round the sun, or the sun has appeared to move round the earth, until the line of apses is at right angles to the line joining the earth to the sun, or when the apses are "in quadratures." In this case, in the neighbourhood of the apses, we have seen that the perturbing force along the radius vector acts inwards, and the perturbing force perpendicular to the radius vector causes the moon to move slowly at perigee and apogee; therefore the forces have in this case exactly the opposite effects to those we have been considering, and therefore the perigee on the whole regresses.

The question now is, whether the progression when the line of apses passes through the sun, or when the apses are "in syzygy," is greater or less than the regression when they are in quadrature.

Now, the force perpendicular to the radius vector is almost exactly the same, whether the apses be in syzygy or in quadrature; the difference, therefore, in the motion of the perigee in the two cases depends on the forces along the radius vector. Now, we have already seen that this force at quadrature is much less than it is at syzygy, therefore the effect when the apses are in syzygy will outweigh the effect when they are in quadrature, and therefore the perigee will on the whole progress.

Hence we see, what we should never on the face of it have expected, that the progression of the moon's perigee, discovered of old by Hipparchus—and which is still true when we know that the moon's orbit is an ellipse with the sun in the focus, and not an eccentric, as he thought—is a consequence of the law of gravitation.

But when Newton calculated the rate of this progression, he obtained a result only half as great as the observed rate. This discrepancy remained long a difficulty in the way of completely accepting the law of gravitation.

And it is important to remember that this progression of the perigee is not continuous, but that it sometimes progresses and sometimes regresses.

It is obvious, from the definition of an ellipse given above, that there may be different sorts of ellipses—some round, some elongated, and this elongation may be measured by the distance of the focus from the centre in proportion to the length of the ellipse, or (Fig. 13) by $\frac{SC}{AC}$; this quantity is called the "eccentricity" of the ellipse. Let us now inquire what the effect of the perturbing force of the sun is upon the eccentricity of the moon's orbit.

Now, it may be calculated mathematically, though the proof is too technical to give here, that if a body be projected into space under the attraction of another body, the length of the major axis of the ellipse which it describes will depend solely on the velocity of projection and on the place from which it is projected, and not at all on the direction of projection.

Now let us consider a body moving in an elliptic

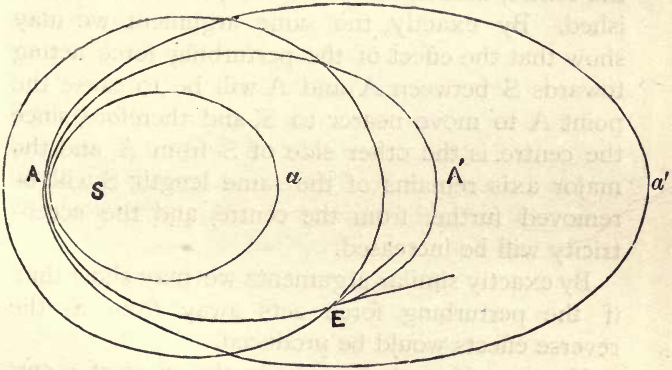

FIG 20

orbit to be perturbed by a force acting inwards along the radius vector. If S (Fig. 20) be the attracting body in one of the foci, let the perturbing force act for a little while when the body is at E, any point at which it is moving away from S. Since the force acts inwards, it will increase the attraction towards S, and therefore will bend the orbit round more at E than would have been the case if there had been no perturbing force; therefore, without altering the velocity, we may consider that the direction of projection at E will be more nearly inclined to S, and therefore the point at which the body will be furthest from S will be brought nearer to S. But since the velocity will not be materially altered, the major axis will remain nearly the same, and therefore, since A′ is brought nearer to S, S must be brought nearer to the centre, that is, the eccentricity must be diminished. By exactly the same argument we may show that the effect of the perturbing force acting towards S between A′ and A will be to cause the point A to move nearer to S, and therefore, since the centre is the other side of S from A, and the major axis remains of the same length, S will be removed further from the centre, and the eccentricity will be increased.

By exactly similar arguments we may show that, if the perturbing force acts away from S, the reverse effects would be produced.

Now consider what would be the effect of a perturbing force acting perpendicularly to the radius vector. Suppose the body to be at A. If the force acts in the direction in which the body is moving, it will increase its velocity, the attraction of S will not be able to drag the body round into so curved an orbit. Its new orbit will therefore lie outside the old, and therefore the new A′ will lie at *a*, further from S than A′ is; hence the distance of S from the centre is increased in a greater proportion than the distance of A from the centre, and therefore the eccentricity is increased.

Similarly, if the force retards the motion of the body at A, the new A′ will lie at *a′*, inside the old orbit, and the eccentricity will be diminished.

By exactly similar reasoning we may see that, if the perturbing force helps the motion at A′, the eccentricity will be diminished; if it retards it, the eccentricity will be increased.

Now let us apply these results to the motion of the moon.

Suppose the line of apses to pass through the sun, then the perturbing force along the radius vector sometimes acts outwards and sometimes inwards; but the magnitudes of these forces are exactly equal in corresponding portions of the orbit on either side of the line of apses, and therefore the results we have arrived at above show that these effects counterbalance each other, and the eccentricity will on the whole not be altered. In the same way the force perpendicular to the radius vector increases the velocity from M_4 to M_1 (Fig. 17), and diminishes it by the same amount from M_1 to M_2. Hence, if M_1 be perigee or apogee, the effect of the force on the eccentricity from M_4 to M_1 will be exactly balanced by its effect from M_1 to M_2. And in the same way the effect from M_2 to M_3 will balance the effect from M_3 to M_4.

By similar reasoning it may be shown that, if the line of apses is at right angles to the line joining the earth to the sun, it divides the orbit into two similar portions, at similar points of which the effects on the eccentricity are of equal magnitude but of opposite kind. Hence these effects will again balance, and the eccentricity will not on the whole be altered.

But when the line of apses is in some intermediate position, these effects will not balance.

Suppose the line of apses to lie in such a position that the moon passes the apse before it comes into its syzygy nearest to the apse, as in Fig. 21. Now in that case $E M_2$ is greater than

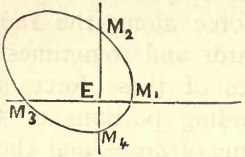

FIG. 21

$E M_4$, and therefore the perturbing force is greater at M_2 than at M_4, and also, by Kepler's second law, the moon is moving slower at M_2 than at M_4; therefore the effect produced at M_2 will preponderate over that at M_4. Similarly, the effect at M_3 will preponderate over that at M_1. Now, at M_2 and M_4 the perturbing force along the radius vector acts inwards, and therefore, since the moon is moving from perigee at M_2 and towards perigee at M_4, the eccentricity will be diminished at M_2 and increased at M_4; therefore on the whole, since the effect at M_2 preponderates, it will be diminished. Similarly, the combined effect at M_1 and M_3 will be to diminish the eccentricity, because M_1 may be considered as near perigee and M_3 near apogee.

Now consider the force perpendicular to the radius vector. From M_1 to M_2 it accelerates the motion. But the end of the minor axis lies in this part of the orbit; therefore during one part of the course from M_1 to M_2 the moon must be considered as near perigee, and in the other as near apogee, and therefore the effects produced here will be of an opposite kind, and the result will be small. Similarly, the result is small in the portion from M_3 to M_4. But in the portions M_4 M_1 and M_2 M_3 the motion is accelerated, and therefore, M_4 M_1 being near perigee, the eccentricity is increased, and from M_2 to M_3 it is diminished. But the force from M_2 to M_3, being proportional to the distance from E, is greater than the force from M_4 to M_1, and therefore the effect produced while the moon moves from M_2 to M_3 will preponderate, and the eccentricity will on the whole be diminished.

Therefore, on the whole, the eccentricity of the moon's orbit will be diminished when it is in the position of Fig. 21 with respect to the line joining the earth to the sun.

By exactly similar reasoning we may show that the effect produced on the eccentricity when the moon's orbit is in such a position that she passes syzygy before she comes to the nearest apse, as in Fig. 22, is the opposite to that hitherto found, and the eccentricity is therefore increased.

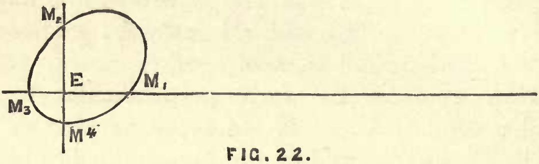

FIG. 22.

As, therefore, the earth, carrying the moon with her, passes in her orbit round the sun from the position in which the line of apses of the moon's orbit coincides with the line joining the earth to the sun into the position in which it is at right angles to it, the eccentricity of the moon's orbit is constantly diminishing; and in passing from the position in which the line of apses is at right angles to the line joining the earth to the sun to that in which they coincide, the eccentricity is constantly increasing;—therefore the eccentricity is greatest when the line of apses points to the sun, and is least when it is at right angles to the direction of the sun.

The alteration produced in the moon's motion by this perturbation, together with that introduced into it by the irregularity in the motion of the perigee, is found by calculation to be exactly the same both in kind and in degree as that inequality discovered by Ptolemy and called the evection. The calculation of it is, however, difficult, and it was only very roughly treated of by Newton.

The remaining two inequalities in the moon's motion which we have hitherto met with—the variation and the annual equation—both discovered by Tycho Brahé, admit of a much easier explanation than the evection.

We have found that the moon is moving faster than her mean motion in syzygies, owing to the perturbing force of the sun, and slower in quadratures. There must, therefore, be four points, one between every two adjacent syzygy and quadrature, at which the moon is moving with her mean or average motion. These points are called octants, and are represented by the points A B C D (Fig. 23). From

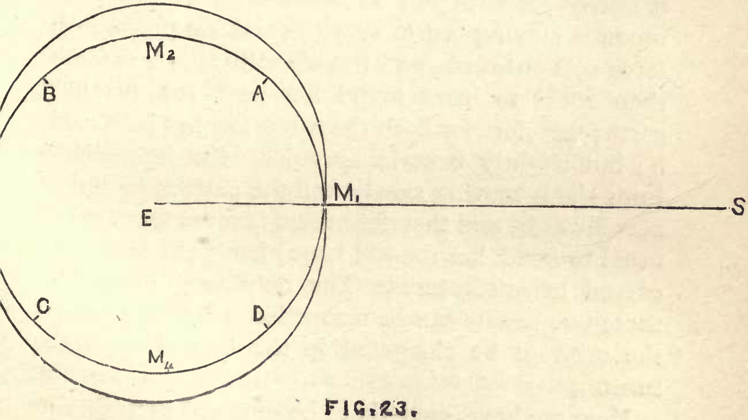

FIG. 23.

D to A, then, the moon is in front of her calculated place, from A to B behind it, from B to C in front of it, and from C to D behind it; that is, she is in front of her calculated place in syzygy and behind it in quadrature, which is exactly what Tycho Brahé observed to form the variation. The amount of this inequality, calculated from the law of gravitation, agrees with observation.

Now, from the fact that the perturbing force acts outwards at syzygy and inwards at quadrature, we might expect the orbit to be elongated in the line of syzygies and flattened in the line of quadratures; as a matter of fact, exactly the opposite is the case, for the following reason:—We can easily observe that, if we throw a stone fast in a horizontal direction, its path will be bent round by gravity very little and it will go nearly straight; but if we throw it slowly, its path will be much curved. Now, the moon is moving fast in syzygies and the perturbing force acts outwards, and therefore there is less force than usual to bend round her path towards the earth; therefore for both these reasons her path will be but slightly bent in syzygies. But in quadratures she is moving slowly and the perturbing force acts inwards, and therefore there is more force than usual to bend her round; hence her path is much curved in quadratures. The only way in which these two results can be reconciled is by supposing the orbit to be elongated in the line of quadratures.

Now, we have seen that the outward perturbing force near syzygy is greater than the inward near quadrature; this fact, combined with the elongation just noticed, will cause the moon's orbit to be on the whole larger than if the sun were not in existence. The combined effect of this enlargement of the moon's orbit, and of its elongation in the direction at right angles to the direction of the sun upon the position of the moon at any instant, produces the variation. Now, it can be easily calculated that these perturbing forces will be greater, and their effects will be greater, the nearer the sun is to the earth and moon. And the earth, moving round the sun in an elliptic orbit, carries the moon with her sometimes nearer the sun, sometimes further off. When, therefore, they are nearer, that is, in winter, the moon moves in a larger orbit than the average, and similarly in summer she moves in a smaller. Hence, being at a greater distance, the force towards the earth will be less, and she will move slower in winter than in summer. This explains that inequality called the annual equation.

Hitherto we have assumed that the plane of the moon's orbit passes through the centre of the sun; but this is not exactly true. We have seen that the sun pulls both the earth and the moon along lines which converge upon his centre. Hence there is a small perturbing force on the moon tending to bring her nearer to the line joining the earth and the sun, tending, that is, always towards some point in the ecliptic. It might be expected from this that the plane of the moon's orbit would gradually shut down upon the ecliptic. This, however, is not the case.

Suppose the parallelogram (Fig. 24) represent a square portion of the plane of the ecliptic seen slightly edgeways; let P M Q N denote the

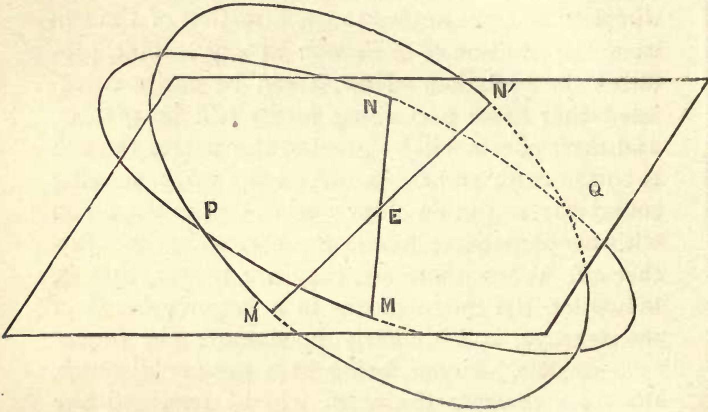

FIG. 24.

moon's orbit, E the earth, and M N the line of nodes. If this perturbing force begin to act at P, it will cause the moon to move more steeply towards the plane of the ecliptic, and to meet it at M′ sooner than it otherwise would. But when it reaches M′ it will have, owing to this perturbing force, a greater velocity resolved perpendicularly to the plane of the ecliptic than it otherwise would have, and therefore, when it has passed through the plane, the whole of the perturbing force that now begins to act in the opposite direction, that is, always towards the plane of the ecliptic, will be needed to counteract this extra velocity, and therefore the moon will move just as far below the plane of the ecliptic as if no perturbing force acted. Hence the inclination of the moon's orbit to the ecliptic will not be permanently affected, but the node will have regressed from M to M′. And since the perturbing force is continually acting, the line of nodes will continually regress on the plane of the ecliptic. The rate of regression, calculated from the law of gravitation, agrees with observation. But we have not space to give here a complete explanation of this phenomenon. There are certain minute irregularities in the motion of the nodes and in the inclination which depend upon the position of the perigee, but which we are obliged to omit.

6. We are now led to the final step of the great inductive ascent. So far we have found that, if a centre of force exist at the centre of every body of the solar system; and if we suppose each of these centres of force to have, as it were, a certain strength, which we call its mass; and if all these centres attract each other, the force between any two being proportional directly to the product of their masses and inversely to the square of the distance between them;—then, if we suppose each body of the solar system to have been projected with a certain velocity, in a certain direction, from a certain place, all the phenomena of their motions which we observe will take place. What we have now to show is that these centres of force are the result of the fact that every particle of matter in

the universe attracts every other particle according to this law.

Newton first showed that if we have any sphere of matter, and we imagine it to be divided up into a vast number of extremely small equal particles; then, if the mass of each of any set of these equal particles which is arranged in a sphere concentric with the sphere of which they form a part, is the same, the resultant of all the attractions of all the particles of the sphere upon any external particle of matter, if they follow the law of gravitation, will be the same as if the whole matter of the sphere were collected together at its centre, and were then treated as a single particle, of mass equal to the sum of the masses of all the particles.

Next he showed that, according to the laws of motion, if the earth be rotating she would assume a form not perfectly spherical, but would bulge out somewhat round the equator, owing to the fact that every particle would try to fly outwards along the straight line in which it is moving at any instant.

Now, we may consider the earth, being of this oblate form, to consist of two parts—one spherical, and coinciding with the surface of the earth at its poles; the other a closed figure, whose inner surface is spherical, and whose thickness is zero at the poles but increases towards the equator, where it is greatest. (A section of this through the earth's axis is represented in Fig. 25.) Now consider any one particle of this near the equator: it may be considered as describing a circular orbit once every day about the point where the plane of its daily path meets the axis. Since the axis of the earth is not at right angles to the ecliptic, the plane of the path of this particle is inclined to the ecliptic, and therefore, as in the case of the moon, its line of nodes will regress on the plane of the ecliptic. Now consider a ring of such particles surrounding

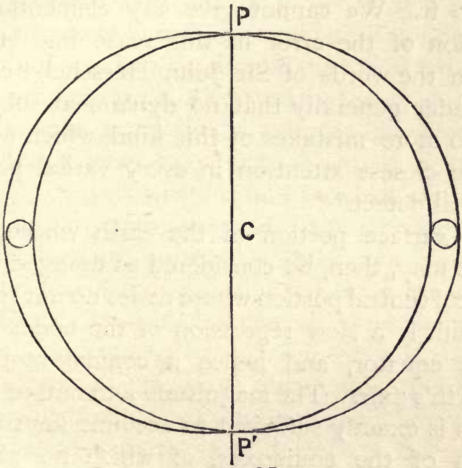

FIG. 25.

the earth's axis: the nodes of every one of these particles will regress, and therefore the nodes of the whole ring will regress. As we may consider the whole of this surface portion of the earth to be divided up into such rings, we see that, so far as this portion of the earth's mass is concerned, the nodes of the equator will regress. The attraction of the moon has a still greater effect in producing this regression than that of the sun.

But the nodes of the equator of the central spherical part will not regress, for we have seen that the attraction on it acts wholly through its centre. It may, however, be objected to this that we can conceive the spherical central mass to be divided up into rings, and that the argument applies equally well to it. We cannot give any elementary explanation of the error in this reasoning, but can only, in the words of Sir John Herschel, "caution the reader generally that no dynamical subject is open to more mistakes of this kind, which nothing but the closest attention in every varied point of view will detect." *

The surface portion of the earth whose nodes regress may, then, be considered as dragged by the spherical central portion whose nodes do not regress; the result is a slow regression of the nodes of the earth's equator, and hence a conical motion of the earth's axis. The magnitude and nature of this motion is exactly sufficient to account for the precession of the equinoxes, of which no physical explanation had been even suggested since its discovery by Hipparchus, eighteen centuries before.

One other entirely original explanation of a phenomenon which had baffled the ingenuity of all previous astronomers resulted from the assumption of the truth of this sixth step in the proof of the law of gravitation. It had been long observed that

* "Outlines of Astronomy," § 647 note.

the interval of time between any two consecutive high tides at any place was half the interval between any two consecutive transits of the moon across the meridian. Newton explained this as follows:—Any particle of water may be supposed to be a small satellite describing a circular orbit about that point where the earth's axis meets the plane of the latitude of the place; hence it will suffer a perturbation of its path by the moon similar to the moon's variation, but its orbit will not be elongated at right angles to the line joining the earth to the moon, because that effect was produced by the acceleration of the motion of the perturbed body as it approaches the line joining the perturbing body and the body round which it is revolving, and by a retardation of its motion as it leaves that line; but a particle of water is not perfectly free to move—it is surrounded by other particles, and those which are approaching the line joining the earth and moon are accelerated, those which are leaving it are retarded. The result is a heaping up of the waters in the line joining the earth and moon, both towards the moon and away from it. The earth, therefore, carries every part of itself twice through a high tide in one revolution with respect to the moon. This is true of the open sea; but the actual motion of this heaping up of the waters depends upon the form of the coast for all places by the seaside. The interval of time between consecutive high tides is, however, the same at all places.

The sun has a similar influence on the ocean, and thus there is a solar tide; but, owing to its great distance, it is but small.

It follows, then, that it is only if we assume the force of gravitation not to reside at the centres of the bodies of the solar system, but to reside in every particle of matter, that these two phenomena of the precession and the tides can be explained. We are therefore led to believe in the complete truth of the law of gravitation; and the investigations of succeeding centuries have only served to verify it.

Two new discoveries followed as a consequence of the views expressed in the "Principia."

In the first place, the orbits of the comets were supposed to be very elongated ellipses, and this supposition was found to agree with their observed motions.

In the second place, Newton appears to have been the first to turn the objection to the earth's rotation, derived from the motion of a falling body, into a strong argument in its favour. For a body on the top of a tower is carried round a larger circle in a day than a body at its foot, and therefore has a greater eastward velocity; hence, by the laws of motion, it ought, in the time it takes to fall, to move more in an eastward direction than the foot of the tower, and therefore to fall a little eastward of it. This was found to be the case, and so the last objection to the Copernikan system was swept away.

Newton seems to have been strangely averse to publishing his work. Perhaps he remembered his contentions with Linus. Certainly he could ill afford the expense. The Royal Society had too liberally assisted less important publications, and was now in want of funds. It should never be forgotten by students of science that we owe to the disinterested generosity of Halley the publication of the "Principia." The minutes of the council of the Royal Society for the 2nd of June, 1686, contain the resolutions, "That Mr. Newton's book be printed," and "That Mr. Halley undertake the business of looking after it, and printing it at his own charge, which he engaged to do." The whole work was given to the world, complete, about midsummer, in 1687.

Unlike most great works of science, the "Principia" was the result of one continuous unbroken effort of thought, and possesses thereby a oneness and perfection which is as striking as its wonderful originality.

When we consider that the accomplishment of any one of the six steps of its grand inductive ascent would have been enough to place its author in the sacred ranks with Hipparchus and Ptolemy, with Kopernic and Kepler and Galileo, we must feel, with Dr. Whewell, that all six steps made at once formed "not a leap, but a flight." And when we consider the state of knowledge in which Newton found the world, and the dim uncertain twilight in which men groped after the ends of truth before the "Principia" was published, and then see the masterly grasp with which he there seized all the facts, and laid the foundations of all physical science so firmly and so well that for two centuries they have never been touched by others but they have been marred, we must know that Pope's epigram is hardly an exaggeration—

> "Nature and nature's laws lay hid in night:
> God said, 'Let Newton be,' and all was light.'

CHAPTER VIII.

ON NEWTON AND HIS FOLLOWERS.

ALTHOUGH, as we have said, the "Principia" was the result of one continuous effort, yet the discoveries which we have endeavoured to explain in the last chapter were not completed, even in the form in which Newton left them, in the first edition of his great work. In particular, the lunar theory was developed throughout the rest of his long life; and the second and third editions of the "Principia" published respectively in 1713 and 1726, contained extensions of it. In order to make these further verifications of the theory, more exact observations of the moon's motion were needed; and to obtain these Newton applied to the Astronomer Royal, John Flamsteed.

This man was born at Denby, near Derby, in the year 1646. Having received an elementary education at the free school of that place, he entered Jesus College, Cambridge, but he seems never to have studied there, nor even to have resided there, with the exception of a few months in 1674. In 1673 he received the degree of Master of Arts at Cambridge.

In 1667 he had explained completely the "equation of time," or the difference between the moment when the sun crosses the celestial meridian of a given place on a given day, and the moment when he would cross it if every solar day of the year were of exactly the same length—a not very difficult deduction from the doctrine of the sphere, which seems to have escaped all previous astronomers, but which we have not space to explain here.

In 1669 he addressed an astronomical paper to the Royal Society, whose authorship, though concealed under an assumed name, was discovered. This brought him into communication with many men of science, especially Sir Jonas Moore, who in 1674 proposed to establish him in an observatory at Chelsea; but the value to the nation of good astronomical observations being brought under the notice of Charles II. by the difficulty of finding the longitude at sea, the king resolved to found the Greenwich Observatory, and Flamsteed was appointed first Astronomer Royal, and took up his abode in the new observatory in July, 1676.

From this time modern astronomical observation may be dated, and the first extensive observations with the help of a telescope were published in Flamsteed's "Historia Cœlestis," in 1725,* after his death, which took place in 1719.

His ingenuity in the improvement of instruments, his scrupulous accuracy, his indomitable perseverance under difficulties, place him beside Tycho Brahé as an observer of the first order; but he differed from the Danish astronomer, both in the greater advantage of possessing telescopes and in the greater misfortunes of poverty and ill health. He was presented to a small living, and his salary as Astronomer Royal was £100 a year; out of this he had to buy and keep in repair all his instruments, and to pay his assistant.

When a boy of fourteen, he caught cold while bathing; this led to a chronic disorder which lasted the rest of his life, and, like Galileo, his observations were conducted amid almost constant physical pain. His maladies led to a peevishness of temperament, which, though for this reason excusable, is nevertheless most painful to observe in all his dealings with Newton.

In the year 1680 two comets appeared. Flamsteed observed their apparent motions, and came to the conclusion that they were one and the same body, which had been lost sight of for a time, owing to its proximity to the sun and its consesequent appearance above the horizon only in daylight. Newton at first maintained they were two,

* In 1712 the first edition of this work appeared, but as it was published by the Government, under the superintendence of Newton and Halley, it was not printed in accordance with its author's wishes, and was disowned by him.

but afterwards saw that Flamsteed was right, and explained the motion of this body by the theory of gravitation. In the mean time, however, he wrote a gentle letter of advice to Flamsteed, suggesting greater care before he insisted publicly on his theory. The peevish mind of Flamsteed brooded over this until he grew to hate its author for "magisterially ridiculing his opinion," as he said.

Later on, when Newton wanted more careful observations on the moon's movements, he applied to Flamsteed, and the correspondence between them on this subject, which began in 1690, illustrates the unhappy character of the Astronomer Royal. He took no interest in Newton's theories, and unnecessarily mortified him by delaying to send the precious observations. A similar delay took place over observations which were needed to verify Newton's tables of refraction.

Centuries before, in the old Greek days, a correct general explanation of this phenomenon had been given. Long after men had come to the conclusion that eclipses of the moon were caused by the shadow of the earth cast by the sun upon the moon, an eclipse of the moon was observed at Syracuse while the sun was still above the horizon. The sun and moon were therefore, in this case, not at exactly opposite points of the celestial sphere, and if the received explanation of eclipses were true, it seemed as if the light of the sun could not pursue a perfectly straight path in passing from him to the moon. It is, perhaps, one of the greatest instances of clearness of conception among the ancient men of science that Archimedes of Syracuse (B.C. 287–212) gave the true explanation. He knew by careful experiment that a ray of light, in passing from one transparent medium into another, will not pursue a perfectly straight course, but will be bent away from the surface separating the two in the denser medium. He therefore supposed that the earth's atmosphere did not fill all space, but that it existed only to a finite height above the earth's surface, and that a ray of light, meeting its upper surface in a slanting direction, would therefore be bent downward towards the earth. An observer, only knowing of the position of a body by the direction in which the light coming to him from it enters his eye, would therefore see all the heavenly bodies in positions nearer the zenith than they actually were.

Willebrord Snell, a Dutch mathematician of Leyden (1591–1626), first gave the true law of the relation between the incident ray and the refracted ray, in 1621. According to this law, the sine of the angle of incidence bears always the same ratio to the sine of the angle of refraction for all directions of the incident ray, when we consider the same two media. In the case of the refraction of the atmosphere, we have not only to consider two media—that through which the ray passes in space, and the air—but we have to notice that the air itself is continually altering in density as we consider it nearer and nearer to the earth's surface; for the air is compressible, and a layer of air near the earth's surface has to support the weight of much more air above it than a layer far removed from the earth's surface, therefore the density of the air will increase towards the earth, and a ray will be bent more and more downwards as it approaches the observer's eye.

The problem, to find the exact amount of the displacement of the apparent position of a star from its true position under these complicated circumstances, was one which Newton solved. And in the eagerness with which he awaited that verification of his theory which Flamsteed's observations alone could give, it is not to be wondered at that he displayed some impatience—an impatience which was more due to his contempt for Flamsteed's paltry conduct than to his own disappointment.

In all his correspondence with Newton, Flamsteed seems to have been actuated by an especial dislike of Newton's friend, Edmund Halley—a dislike for which he repeatedly hints the excuse that Halley was an atheist. As far as we can judge, this belief, which was by no means confined to Flamsteed, was utterly untrue, and arose from the fact that Halley had no mercy for those well-meaning but ignorant and most dangerous people who mistake their own prejudiced interpretation of Scripture for divine truth. Halley used to ridicule these people without stint; but his delight in this sometimes carried him too far, and it is said that Newton, who kept constantly before himself the supreme importance of his faith, used occasionally to rebuke him when in this mood with the words, "I have studied these things; you have not."

Edmund Halley was born on the 29th of October, 1656, at Haggerston, near London, and was the son of a wealthy soap-boiler. Having passed through St. Paul's School, he went to Queen's College, Oxford, in 1673. He soon turned his attention to astronomy, and determined to make a catalogue of the southern stars. For this purpose he obtained a recommendation from Charles II. to the East India Company, and set out for St. Helena in November, 1676.

The rock was not well chosen, as it turned out, for observation; however, in two years he returned, with a catalogue of three hundred and fifty stars which never rise above the horizon in our latitude, and having observed a transit of Mercury across the sun's disc, and the fact that the oscillations of a pendulum increased in duration as he approached the equator.

Halley was a zealous Tory, and among the new southern constellations he placed the "Royal Oak," in return for which Charles II. granted him a mandamus to the University of Oxford for the degree of Master of Arts, which he immediately obtained.

The increase in duration of the oscillations of the pendulum was to be explained by the fact that the earth bulges out round the equator, and therefore the average distance of the particles of her mass would be greater from a point on her surface near the equator than from one far off from it. And hence, if the law of gravitation be true, the force of attraction on any body would be less near the equator than further off; and, since the swing of a pendulum is due to the weight of its bob, it follows that it will swing slower when the force is weaker.

The transit of Mercury suggested to Halley the method of finding the magnitudes of the orbits of the planets which is still used. We have seen how their relative orbits may be found on the assumption that they move in circles at a uniform rate. When Kepler's laws were discovered, a modification of the method had to be introduced to suit the new facts, and by this the relative magnitudes of the orbits, with the positions of their major axes and their eccentricities, could be found. When this was done, all that remained was to find the absolute value of any one magnitude in the solar system, and all the rest would then be known. The distance of the sun in a known position of the earth in her orbit would do; but we have explained, in considering the distance of the moon in Chapter VI., that the indistinctness of his edge and his great distance render the method of parallax inapplicable to him. Halley, however, suggested that these difficulties would be overcome if Venus were observed at the moment of her transit over the sun's disc, and her distance ascertained by the method of parallax. Her edge would be seen distinct and sharp against the bright face of the sun, and her distance would not be a quarter of his. Halley himself did not live to see a transit of Venus, but at the next, which took place on the 5th of June, 1761, most of the Governments of Europe made arrangements for observing it.

Throughout his life he was the constant friend of Newton, and, in many other ways besides the publication of the "Principia," rendered him assistance. He afforded one of the most striking verifications of the law of gravitation by calculating from observations the position and size of the orbit of the comet of 1680, which has ever since been called by his name, together with its velocity, and hence predicting its return in 1758.

His other astronomical works were the explanation of the "trade winds," and the detection of the "long inequality" of Jupiter and Saturn, and the slow acceleration of the moon's mean motion. The two latter we shall consider in the next chapter, when we treat of their explanation by Laplace.

The motion of the trade winds is, however, important, as adding another proof of the rotation of the earth. We may suppose that the sun sends out the same amount of heat in the same time in each of all cones that are of equal size and have their apices at his centre. Now, if we consider such cones having their bases at small portions of the earth's surface, owing to the great distance of their apices at the sun's centre, we may without appreciable error treat them as parallel cylinders.

Consider two such equal cylinders of sun's rays, one meeting the surface of the earth near the equator, the other near the pole ; the first will meet the surface nearly perpendicularly, the second, owing to the curvature of the earth, very slantingly ; hence the first will cover only a small portion of the earth's surface, the second a comparatively large one, and therefore the same amount of heat which will go to raise the temperature of a small surface near the equator, will be spread over a large one near the poles. It follows, therefore, that the air about the equator is hotter than that near the poles, and therefore is expanded, becomes bulk for bulk lighter, and rises, being pushed up by the heavier cold air at the poles which rushes towards the equator. There is, therefore, a continual flux over the earth's surface towards the equator, and an opposite current in the upper regions of the air.

But the directions of these currents are not due north and south ; for the air that starts southward from a point near the north pole has an eastward velocity which would carry it round a small parallel of latitude in a day, but it moves to a point which has a much greater eastward velocity—one that will carry it round a much longer parallel of latitude in a day. The consequence is that the surface of the earth slips under the wind eastwards as it travels south, and hence to an observer it seems to be a north-east wind ; similarly, in the southern hemisphere, it is a south-east wind.

Halley was elected Fellow of the Royal Society in 1679; he was assistant-secretary to that body in 1685, and secretary in 1713. In 1720, after the death of Flamsteed, he was made Astronomer Royal; he was then sixty-four years of age, and yet he determined to make careful observations of the motion of the moon during a complete revolution of her nodes—a period of nineteen years. This Herculean task he just lived to finish, and left as a result of it two thousand observations of material use in the verification of the lunar theory. He died on the 14th of January, 1742.

Halley's successor as Astronomer Royal was James Bradley, who had already added to astronomical knowledge two important discoveries. His appointment was the very last act of Walpole's administration.

Bradley was born at Sherbourne, in Gloucestershire, in 1692. In 1710 he entered Balliol College, Oxford, and took his degree in 1714. In 1721 he was appointed to the Savilian Professorship of Astronomy. It had long been known that some of the fixed stars altered their apparent positions on the celestial sphere by a quantity which was appreciable when telescopes were used. In particular, Picard found the pole-star to change through about forty seconds in the course of a year. Hooke and Flamsteed attributed this to the fact that the star had a parallax of this amount, the diameter of the earth's orbit being the base. Others suggested difficulties in the way of this explanation. Bradley and Samuel Molineux, who were then living at

Kew, determined to conduct a series of accurate observations in order to solve this question.

After Molineux was made a Lord of the Admiralty, Bradley continued his researches alone, at Wanstead. After seven years of careful observation, it was found that the star γ Draconis appeared to describe annually a small ellipse in the sky, the major axis of which was about forty seconds. Afterwards it was found that this was the case with all the stars, the eccentricity of their ellipses being greater or less according as they were situated nearer to or further from the ecliptic, and their major axes always being of the same length.

Bradley pondered over the explanation for a long time without success. The solution which he at last found illustrates the way in which one branch of physical science often comes unexpectedly to the help of another. Olaus Roëmer, a Danish astronomer (born 1644; died September 19, 1710), had observed that the moments when the satellites of Jupiter pass into his shadow and are eclipsed take place before their calculated time when the earth is in that part of her orbit which is near to Jupiter, and after their time when the earth is far from Jupiter. He explained this by supposing that light took a finite time to travel, and therefore that, when the earth was far from Jupiter, the observer saw the eclipse at a greater time after its actual occurrence than when she was near to him. And knowing the difference between the distances of the earth from the satellite of Jupiter at the moments of two observations of its eclipses, and the error in time between the two observations, he could calculate the velocity of light to be about 180,000 miles a second.

Bradley knew of this discovery, and, when he least expected it, it came to his assistance. It is said that, while pondering over the annual displacement of the fixed stars, "he accompanied a pleasure-party in a sail upon the river Thames. The boat in which they were, was provided with a mast that had a vane at the top of it. It blew a moderate wind, and the party sailed up and down the river for a considerable time. Dr. Bradley remarked that, every time the boat put about, the vane at the top of its mast shifted a little, as if there had been a slight change in the direction of the wind. He observed this three or four times without speaking; at last he mentioned it to the sailors, and expressed his surprise that the wind should shift so regularly every time they put about. The sailors told him that the wind had not shifted, but that the apparent change was owing to the change in the direction of the boat, and assured him that the same thing invariably happened in all cases." * Here was an exact analogy to his observation on the stars. The boat corresponded to the earth, the wind to the light, and the vane to the telescope. The apparent direction of a star must be found by combining the velocity of the

* This story rests on the authority of Dr. Robison.

earth with the velocity of light by the parallelogram of velocities.

In Fig. 26, suppose waves of light to be moving along the ray B A, we might think that, in order to see the star that is sending out the waves, we ought to point the telescope along the line A B. But while a particular wave of light is moving

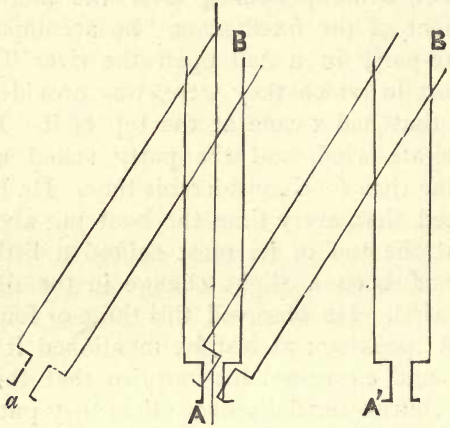

FIG. 26.

from B to A, the earth moves so as to carry the telescope from A B to A′ B′; and so, when the wave would be at A, the observer's eye is at A′. As this is true for every wave, the observer will not see the star, its light all having broken against the side of the telescope. But if the telescope be slanted in the direction in which the earth is carrying it, in some such position as *a* B, then, while the wave moves from B to A, the telescope moves from *a* B to A B′, and the wave which entered the telescope at B is seen by the observer at A. But all he knows about the direction of the star is the direction in which he has to point his telescope in order to see it, and therefore the apparent position of the star is displaced out of its true position in the direction in which the earth is moving.

As the earth moves much more rapidly in its orbit than it rotates, it is its orbital motion which most influences this "aberration" of the stars. And since the earth moves in every direction in the ecliptic in the course of a year, a star out of the ecliptic will be displaced from its true position in every direction in the course of a year, and will seem to describe a small ellipse annually.

Knowing the velocity of the earth at any moment, he could calculate what must be the velocity of light in order to produce the observed displacement, and he found it to agree with Roëmer's result.

The discoveries of Bradley led men to regard minute accuracy in science as of much greater importance than they ever had done before, and Newton used to speak of him as "the best astronomer in Europe." But another discovery was soon made of minute extent, and, perhaps, we may say, *therefore* of great importance. When Bradley attempted to verify his theory of aberration by observations on other stars, he found he could not do so. But after nineteen years of careful work, he discovered that the error from their displacement as predicted by his theory was periodic, that it increased for rather more than nine years, and then decreased by the same amount, the error vanishing about every nineteen years.

After various attempts to account for this, Bradley found that it might be explained by supposing the earth's axis to make little nods of nineteen years' duration—to move, that is, not over the surface of a cone, as it was supposed to do by Newton, in his explanation of precession, but over the surface of a fluted cone.

But this theory seemed to be inconsistent with the consequences of the law of gravitation as explained in the "Principia." And it was only after careful verification that Bradley at last was forced to accept it.

Newton was still living, full of years and of honours, the idol of his own country and the admiration of Europe; and Molineux, who thought that this discovery of the "nutation" of the earth's axis completely overturned the Newtonian system, was sent with some trepidation to break it to the old man. But all Newton replied to him was, "It may be so; there is no arguing against facts and experiments."

This carelessness for "fame," for aught but truth, is typical of his whole life. And in face of facts like these we cannot agree with the assertion which it has become of late somewhat the fashion to make, that Newton's humility and unselfish neglect of his own claims as a discoverer are a myth. On two occasions he found himself in a controversy with another claimant to a discovery really his own, and on both of these he put forward his own claim with some warmth. But in both of these the conduct of his opponents was such as to deserve severe rebuke from an upright man, without any respect to personal considerations, and in both his opponents were the aggressors.

The first was when Hooke claimed to have proved the law of the inverse square for the sun attracting each planet at different points of its orbit. It appeared afterwards that Hooke's conduct had been exaggerated to Newton, and, on learning this, Newton at once withdrew his rebuke.

The second was in the long and bitter controversy with Leibnitz as to the first invention of the infinitesimal calculus. And in that, not only was Newton right in his contention, but he made his claim with the utmost calmness and dignity and with the most transparent honesty; while Leibnitz was guilty, not only of doubtful intrigues, but of conduct flagrantly dishonourable. It seems, therefore, absurd to deny the candour and humility of Newton's character because he retorted on this assailant in no measured language.

Most of the great predecessors of Newton passed through the ordeal of misfortune and failed not. His own lot was far different. And yet, perhaps, his trial was even more searching. To emerge at the end of a long life of unbroken success, of continuous prosperity, with heart still warm; to endure for half a century wealth and honours, the enthusiasm of his countrymen, the admiration of all men, and still to keep sympathy alive, and still to hold fast his faith, is a stronger test of the stuff a man is made of than the steadfast endurance of suffering or the dangerous witness to truth.

And Newton's cup of happiness, as men reckon happiness, was filled to the brim. In closing the enumeration of the great names that lie upon the roll of the Convention Parliament of 1688—that Parliament that created the constitution that has grown into the one under which we now live, Macaulay says, "One other name must be mentioned —a name then known only to a small circle of philosophers, but now pronounced beyond the Ganges and the Mississippi with reverence exceeding that which is paid to the memory of the greatest warriors and rulers. Among the crowd of silent members appeared the majestic forehead and pensive face of Isaac Newton. The renowned university on which his genius had already begun to impress a peculiar character, still plainly discernible after the lapse of more than a hundred and sixty years, had sent him to the Convention, and he sat there, in his modest greatness, the unobtrusive but unflinching friend of civil and religious freedom."

Yet Newton was by no means "silent" when occasion needed that he should speak. In 1687, while he was writing the third book of the "Principia," James II. began the arbitrary course which led to the Revolution. One of his first acts was to demand that the University of Cambridge should confer the degree of Master of Arts on Francis, a Benedictine monk, without his taking the oaths of allegiance and supremacy, according to law. This test, which afterwards became an instrument of injustice, was in those critical days a bulwark of liberty. The university took its stand upon law, and refused to obey the order. A menacing letter was sent by the Government, and the university was summoned to appear by deputies before the new High Commission at Westminster. The Vice-Chancellor, Newton, and eight others were appointed by the Senate to represent the university. At a preliminary meeting of these deputies, some advocated concession and compromise; but Newton rose and spoke so strongly for determined action that they stood firm, and, before the quarrel came to a crisis, the Revolution freed the university from danger.

As the "Principia" was more and more read, the reputation of its author became more and more famous, until at last men began to feel that some mark of national gratitude should be conferred upon him, and several of his friends exerted themselves in his behalf; among them was John Locke; but they were not at first successful.

In 1694, however, Charles Montague was made Chancellor of the Exchequer, and was thus brought face to face with the necessity of dealing with the debased currency by recoinage. Montague was about twenty years younger than Newton, and had been one of his most ardent disciples at Cambridge; and knowing that his income was not large, and being determined, as he said, "that he would not suffer the lamp that gave so much light to want oil," he decided to make Newton Warden of the Mint. The office was no sinecure, and all Newton's great reputation was needed to support the proposition for recoinage, which, like all great reforms, met with bitter opposition from interested men.

In 1699 the Mastership of the Mint fell vacant, and Montague, who was now First Lord of the Treasury, raised Newton to that important office, with a salary of £1500 a year. This was one of the offices in which it was ordinarily recognized that a man might enrich himself, by specially obliging individuals in a way we should now call dishonest. But Newton had an ideal before him which he would follow without regard to precedent. On one occasion he was offered £6000 by a petitioner, to give him some advantage which it was in his power to grant. Newton refused on the ground that it was a bribe. The man then informed him that he was the agent in this matter of a "great Dutchesse," and pleaded her quality and interest; but Newton roughly answered, "I desire you to tell the lady that if she were here herself and had made me this offer, I would have desired her to go out of my house; and so I desire you, or you shall be turned out." This novel honesty led him into some controversies at the Mint; he, however, seems soon to have escaped them, and from that time forward honours came thick upon him.

In the same year he was appointed one of the eight foreign associates of the French Academy of Sciences. In 1703 he was elected President of the Royal Society. The office was, and is, ordinarily only held for five years, but Newton was re-elected until the end of his life. In this position he was brought into constant intercourse with the court. In the month of April, 1705, we find him at Cambridge again, while Queen Anne, with her husband, Prince George of Denmark, were holding their court at Newmarket. On this occasion they paid a visit to Cambridge, and, amongst various distinctions conferred upon the chief officers of the university, the Queen conferred the honour of knighthood on the most illustrious of her subjects, Sir Isaac Newton.

The rest of his life was spent in quiet and uneventful dignity in London, only disturbed by his controversies with Flamsteed and Leibnitz, and by theological publications and discussions. Two of the latter related to corruptions of the text of Scripture, both of which concerned the doctrine of the Trinity. Consequently the great name of Newton has been claimed by the Arians and the Socinians as that of an ally. There can be no doubt that this is a mistake; and passages in many of his private letters, as well as his public profession, prove that he held with the simplest faith the two great central doctrines of Christianity—the Incarnation and the Atonement.

"That which is not clear is not French" is, we think, a fair boast; but then the retort must be also fair that therefore the French intellect is incapable of grasping those deeper mysteries of life which, by their very nature, cannot be clear to us in the present condition of our faculties. It is not surprising, therefore, that some French biographers have not been able to explain the simple faith of the great man on any other hypothesis than that his theological studies were undertaken in his dotage. There is no doubt, however, that this is not the truth. From his youth he had searched the Scriptures, and there is certain evidence that one of his chief theological works was written before 1690, when in the very acme of his powers.

And his religion was no mere profession. His gentle and generous character won the love of all who knew him, as his stupendous powers compelled their admiration.

During these latter years he was not neglectful of his mathematical studies, and although his extreme conscientiousness led him to put them off, saying that "he must be about the king's business," yet he continued to improve the lunar theory, and to help mathematicians throughout Europe in their difficulties. On one occasion Leibnitz stated, in answer to the Queen of Prussia, "that, taking mathematicians from the beginning of the world to the time when Sir Isaac lived, what he had done was much the better half; and added that he had consulted all the learned in Europe upon some difficult points without having any satisfaction, and that when he applied to Sir Isaac, he wrote him in answer, by the first post, to do so and so, and then he would find it." * On another occasion he received a problem one evening which had puzzled all the chief mathematicians of Europe for six months, and the following day he sent off the solution. In 1716, when seventy-four years old, Leibnitz proposed a problem to "feel the pulse of the English analysts." Newton received it at the Mint at five o'clock in the afternoon, and, though fatigued with business, he solved it before he went to bed.

And yet Dr. Pemberton, who as a young man prepared for him the third edition of the "Principia," writes, "This I immediately discovered in him. . . . Neither his extreme great age nor his universal reputation had rendered him stiff in opinion or in any degree elated. . . . The remarks I continually sent him by letters on the 'Principia' were received with the utmost goodness."

Mr. Conduitt describes his appearance in these days. "He had a very lively and piercing eye, a comely and gracious aspect, with a fine head of hair as white as silver, without any baldness, and, when his peruke was off, was a venerable sight."

* Quoted from the Conduitt MSS., in Brewster's "Memoirs of Newton," ii. 406.

Up to the very last Newton seems to have possessed his faculties in full vigour. On the 2nd of March, 1726, he presided at the Royal Society, but next day he was taken ill, and on the morning of Monday, the 20th, what Bishop Burnet called "the whitest soul he ever knew," passed away from among men.

Looking at the work he did, we can scarcely forbear to think of Newton as the only astronomer; we honour him too unjustly in this. The little work of careful observation had to be done before the great theory could be constructed; much laborious digging in the secret foundations needs to be worked through before the stately palace can be built; and knowledge came not like the blinding lightning, but rose like the morning, upon men—first the faint glimmer of light in the east, slowly broadening and brightening to the west, until the eyes, weakened by long darkness, could bear to look upon nature illumined by the sun that rose with Newton. Let us not forget this; let us look upon the story of science in the spirit of his own wonderful words, "If I have seen further than other men, it is because I have stood upon the shoulders of the giants."

England for a space forgot this, and we can see a presage of the coming time in Newton's funeral. His body lay in state in the Jerusalem Chamber, with honours otherwise only paid to kings, and was buried in the great mausoleum of the nation, in a place refused hitherto to England's greatest rulers; the funeral was a national one, and six peers (!) bore his pall. The faithless eighteenth century and all its works have passed away, and "six peers" would scarcely now be thought peculiarly worthy to bestow honour on one of the kings of patient labour and God-given genius; yet let us give the age its one credit of having recognized the last great man of the seventeenth century, and having honoured him with the best it had to give.

From this time astronomy passed from the guardianship of England until the days of men still living. Newton had used the old synthetic method in mathematics, and his English followers did not see that it was one of his greatest triumphs to have done what he did with such cumbrous tools. And so for generation after generation they worked away, rejecting the beautiful and powerful instruments of analysis which the French mathematicians were perfecting; and they never added by this means aught to human knowledge except the one theorem of Colin MacLaurin, relating to the attraction of ellipsoids.

Spiritual deadness, intellectual slavishness, political corruption, showed a decay which might have made men despair of the future of their country; and superstition seemed about to revive. When Bradley, in 1752, was concerned in the alteration of style in England, the people believed the astronomer was somehow going to rob them of eleven days of their lives, and his decline and death soon after was popularly supposed to be the judgment of Heaven.

Men followed the letter of Newton's work, and forgot its spirit; they looked back with admiration upon the past, and not forward with hope into the future; and they understood not the meaning of the wonderful parable, in which he declared his position at the end of his eighty years of labour, that he felt like a little child playing on the seashore: the waves had washed one or two shells to his feet, but there still lay the infinite treasures of the ocean undiscovered.

CHAPTER IX.

ON LAGRANGE AND LAPLACE.

In the period we have just been considering, England stood supreme in science; during the next period that supremacy passed, not less markedly, to France. But the method used was that of mathematical analysis, and of that we can give no explanation in an elementary work like the present—partly because it has to do with perhaps the most abstruse difficulties which the mind of man has attacked, and partly because it merely effects the complete verification of the law of gravitation. The reader, however, must not imagine that the subject is therefore unimportant; the *exact* verification of a theory is all important in science, and those who study the subject of astronomy completely will find that more than half their work is concerned with what we are obliged here to compress into a single chapter.

We have seen that the chief difficulty in the way of the law of gravitation was the rate of progression of the moon's perigee, which Newton had calculated to be only one-half what it was observed to be.

Alexis Claude Clairaut (born Paris, May 7, 1713; died Paris, May 17, 1765) attempted to solve this difficulty. At first he used Newton's methods, and he obtained exactly the same result; afterwards, however, he investigated the question by means of the new analysis. At first he found the same result, and he was led to doubt the exact truth of Newton's law of the inverse square; he supposed that the law was not expressed by the formula $G = \frac{h}{D^2}$, but by $G = \frac{h}{D^2} + \frac{k}{D^4}$, where G is the force of gravity between two particles at distance D from each other, and h and k are two quantities dependent on the masses of the particles which do not alter with the distance, and k is very small. But on reconsidering his reasoning, he found out the mistake, and deduced from Newton's law of gravitation a rate of motion for the moon's perigee that agrees with observation. The mistake had arisen, both in Newton's argument and in Clairaut's first attempt, from an imperfection in the method of approximation used.

As the year 1758 approached, the astronomers awaited with intense interest the return of Halley's comet, which he had prophesied to take place in that year. In his calculations, however, Halley had omitted to take any account of the perturbations it might suffer from the planets it passed on its way. Clairaut saw that it would pass near to Jupiter and Saturn, and calculated, in 1757, that the perturbations would be such as to cause the comet to pass its perihelion on the 13th of April, 1759; but he stated that the intricacies of the calculation obliged him to omit many small terms in his approximation which together might cause an error in the calculation of as much as one month. The event took place just within the limits assigned by Clairaut, for the comet was observed to pass its perihelion on the 13th of March, just one month before its predicted time.

Throughout the eighteenth century the French Academy of Science frequently offered their annual prize for a complete solution of the lunar perturbation, and consequently the attention of mathematicians was constantly directed towards this subject. Newton, as we have seen, had explained by the law of gravitation those chief lunar perturbations which had been previously observed; but he had seen that other minute ones probably existed, and the object of mathematicians now was to find a complete solution of the moon's motion, with all its irregularities, on the assumption of the truth of the law of gravitation. We have already seen that, owing to his great mass, the sun is the only body which need be taken into account as perturbing the orbit of the moon round the earth. Treated by the aid of analysis, the problem becomes one of complete generality, and has no more than an accidental relation to astronomy. In this general sense it is usually called the "problem of three bodies."

It cannot be completely solved, but methods have been invented by which the solution may be approximated to. The first mathematicians to attempt the solution by analysis were Clairaut and Jean le Rond d'Alembert (born Paris, November 16, 1717; died Paris, October 29, 1783). In the course of their labours upon the same subject, a rivalry sprang up between them, which gathered point from the remarkable contrast between their characters. Clairaut was a polished man of the world; D'Alembert, on the other hand, was an honest but rough disputant, whose favourite maxim was, "J'aime mieux être incivil qu'ennuyé."

D'Alembert's greatest work was the rigorous deduction of the phenomena of precession and nutation from the law of gravitation. He showed that the former was due to the attractions of the sun and moon, the latter to the fact that the moon's orbit is not exactly in the plane of the ecliptic, whence the period of nutation is the same as that of the revolution of the moon's nodes.

To these must be added the name of one of the very greatest of mathematicians, Leonard Euler (born Basle, April 15, 1707, died St. Petersburg, September 7, 1783). Euler not only attempted the complete solution of the lunar problem, but was the first to carry the investigation of physical astronomy into regions which Newton had not entered. Newton had seen that, according to the law of gravitation, the planets ought to perturb one another, but so small are they compared with their distances apart, and with the sun, that the inequalities so introduced are extremely minute, and so he contented himself with merely noticing the fact, without calculating their perturbations. Euler, however, attempted to calculate the mutual perturbations of Jupiter and Saturn, and the perturbation of the earth by the other planets; but, although in the course of his work he materially assisted astronomy by improving the analytical method, he did not succeed in the immediate object of his researches.

In 1763 he was, however, more successful in a memoir he communicated to the Academy of Berlin, on the effect of the spheroidal form of Jupiter on the orbits of his satellites. Jupiter, like the earth but to a greater extent, bulges out round his equator, owing to his more rapid rotation; and, as in our explanation of the precession, we may consider him to consist of two parts—a sphere concentric with him and coinciding with his surface at his poles, and an outside layer, thicker at the equator than elsewhere.

The sphere will attract as if its whole mass were gathered at its centre; the outside layer will also attract, but not in this way. Its attraction may therefore be regarded as a perturbing force, acting inwards along the radius vector; and hence, by the reasoning of Chapter VII., the line of apses of each of his satellites will progress when near perijove, and regress when near apojove. If this perturbing force always bore the same proportion to the attraction of the main central spherical portion of the planet in all positions of the satellite, these two would balance, and the apse line would remain stationary; but it does not do so. It can be proved that the *proportion* of the attraction of the outer layer to that of the central sphere varies inversely as the square of the distance of the satellite, and is therefore less near apojove than near perijove, hence the apse line on the whole progresses.

It can be also shown that, if the orbit of the satellite be not exactly in the plane of the planet's equator, that part of the outside layer which is nearer the satellite than the planet's centre tends to draw it towards that plane, and the other part tends to draw it from the plane; the former is the stronger. And then, by reasoning exactly similar to that by which we explained the regression of the moon's nodes, it follows that the nodes of the orbit of the satellite on the plane of the equator of the planet must regress.

All the planets are of an oblate form, hence this reasoning applies to all the satellites. But in the case of the earth, her elipticity is so small that this effect is inconsiderable upon the moon's motion compared with the similar effects produced by the perturbing action of the sun. But Jupiter is so oblate and the sun is so distant that, at any rate in the case of his nearest satellite, the action of the sun is inconsiderable compared with that of the protuberant parts round his equator. In the case of the satellites of Saturn, these effects are still further intensified by the attraction of his ring, whose plane coincides with that of his equator.

In 1744 Euler published a work entitled "Methodus Inveniendi Lineas Curvas Maximi Minimive Proprietate Gaudentes." In this he challenged the mathematicians of Europe to find a general analytical method of solving the questions of which it treated. Many had attempted this work and failed; but in 1755 he received a communication from a man utterly unknown, containing the required method, now known as the "calculus of variations." The author was Joseph Louis Lagrange, and at the time Euler received his paper he was only nineteen years old.

He was born on the 25th of January, 1736, and was a Frenchman by family, though a native of Turin, where his father was paymaster of the forces. When he was quite young, his parents lost all their fortune, and hence he was brought up with the knowledge that he would have to earn his living. It was fortunate for science that this was the case, for he himself afterwards confessed, "Si j'avais eu de la fortune, je n'aurais pas fait mon état des mathématiques." This is more strictly true than such statements generally are, for, though Lagrange progressed with marvellous rapidity when once he had commenced his mathematical studies, it was comparatively late that he discovered his fitness for this kind of research.

At the University of Turin, where he entered at fifteen years of age, the Latin classics were his favourite study. In his second year he commenced the study of mathematics, beginning with the old synthetic methods. But soon he came across a paper of Halley's, in which some elementary applications of analysis were pointed out. This seemed to open up a new field of knowledge to him, and from this time forth all his energies were devoted to the study and improvement of mathematical analysis. In two years he had mastered all that had been previously done in this direction, and was appointed Professor of Mathematics at the School of Artillery at Turin. He was only nineteen years old, and every one of his pupils was older than himself.

About this time he founded the Academy of Turin, and his earliest papers were published among its Memoirs. In 1759 the first volume of these appeared, and was composed mainly of the work of the young mathematician. On the 2nd of October in this year he was elected a member of the Academy of Berlin. In 1762 a second volume of the Turin Memoirs was published, and again it consisted mainly of Lagrange's work on various parts of pure mathematics, and its application to the vibrations of strings. In the volume that appeared in the following year he gave a new solution of the problem of three bodies, and applied it to the particular case of Jupiter and Saturn.

We have said that the planetary perturbations, owing to the small masses and great distances of the planets, are inconsiderable; and, in fact, for a single revolution of a given planet, its perturbation from the motion described by Kepler's laws is so small that no observation, however careful, could detect it. But, just as an inappreciably small alteration in the length of the pendulum of an accurately set clock might produce the very appreciable error of two or three minutes in its time at the end of a year, so in the case of the planets, though these perturbations are inappreciably small for a single revolution, yet it may be that they accumulate through many revolutions and become appreciable. Something of this sort is the case with the "long inequalities" of Jupiter and Saturn, first detected by Halley.

It was found, by comparing ancient observations with modern, that the mean velocity of Jupiter was increasing, and that of Saturn was diminishing; but so slowly, that the apparent position of Saturn was only, in 1716, thirty-four minutes and a half behind the position it would have occupied had it continued, ever since 1598, to move with the velocity it then had. But it was found, by comparing these observations with still later ones, that this inequality had changed its character, and that Jupiter was, at the time Lagrange investigated the subject, being retarded and Saturn accelerated.

Lagrange materially assisted the discovery of the cause of this inequality by his analysis, but he failed to trace it completely from the law of gravitation ; and this, like many other discoveries to which he pointed out the road, was actually made by Laplace.

Pierre Simon Laplace, who must be placed second to Newton alone as a physical astronomer, and who was the author of that great treatise the "Méchanique Cèleste," which must be classed with the "Almagest" of Ptolemy, the "De Revolutionibus Orbium Cœlestium" of Copernik, and the "Principia" of Newton, as among the four greatest works on astronomy, was the son of a poor labourer of Lower Normandy. He was born at Beaumont, near Point-l'Evèque, on the 22nd of March, 1749. He seems to have shown such precocity as a child that some benevolent people wealthier than his own relations sent him to school at Caen. Having completed his course there, he went to the Military School of Beaumont. It was here that Laplace first gave evidence of his great mathematical powers. So far he had been chiefly remarkable for a wonderful memory and extraordinary acuteness in debating points of theology, and it seems that he had been destined for the Church ; but he himself never entertained this idea.

When he had passed through his course as a student at Beaumont, he remained there, in the capacity of Assistant Professor of Mathematics. He now completed his mastery of mathematical analysis and its applications to dynamics and astronomy. He felt, however, that his isolation at Beaumont did not give his great powers due scope ;

and so he set out for Paris, with a letter of introduction to D'Alembert. At first he was not received, and so he addressed to the great man a letter on the principles of dynamics, which so impressed him that D'Alembert procured for him a mathematical chair at the Military School of Paris. He was not long in justifying his appointment; for in 1772 he communicated two papers of great value to the Academy of Sciences, on the differential and integral calculus, and their application to the planetary theory. But from this time his work went on step by step with that of Lagrange. We must, therefore, return to consider the course of his life since we left him in 1763.

In 1764 Lagrange gained the prize of the Academy of Sciences of Paris for a memoir on the libration of the moon. The librations we have so far considered have been apparent, not real, and are due, as we have explained, to the inequalities in the revolution of the moon, combined with her uniform rotation in the same period. But Lagrange, in this memoir, pointed out a physical libration, an actual oscillation in her rotation. He showed that the moon's rotation, although so slow, would cause her to bulge out slightly round her equator, and that, just as the moon causes the tides in the earth's ocean, so the earth would drag the moon into an elongated form, whose elongation would point towards the earth's centre. This elongation would not travel through the mass of the moon, as the tides pass through the ocean, because the moon always presents the same face to the earth. We must, therefore, regard the moon as an ellipsoid, whose longest axis passes through the earth, and its shortest is the moon's axis of rotation, nearly perpendicular to the ecliptic, the mean axis being perpendicular to these two. Lagrange then showed that the attraction of the earth would always keep the longest axis towards the earth's centre, so that when in her revolution this axis is turned a little to one side, the earth's attraction pulls it back again, and thus keeps up a constant small oscillation. He further proved that, if ever the moon was brought into a rate of rotation nearly equal to her mean rate of revolution, this attraction for her longest axis would bring the two rates into exact equality—a result which we shall find in the last chapter to be of some importance.*

In 1766 Lagrange again won the prize of the Academy for a paper on the theory of Jupiter's satellites, the mathematical difficulties of which he overcame; but he again failed to arrive at a satisfactory result, and the completion of his labours was again left to Laplace.

In this year he left Turin. Euler had gone back to St. Petersburg, and resigned the directory of the Academy of Berlin. Frederick the Great tried to get D'Alembert to fill his place, but he refused, and suggested Lagrange. Lagrange was invited and desired to accept the invitation, and applied for leave to the King of Sardinia. An audience was

* *Vide* p. 321.

granted him, but his request was refused. As he was leaving, the king said he should like to see his letter of invitation. The letter contained the words, "The greatest geometer of Europe should be near the greatest of kings." Seeing this, the king's pride was offended, and he passionately exclaimed, "Go, then, sir; go, and attach yourself to the greatest king in Europe;"—a good illustration of the childish caprices which the subjects of hereditary power have to suffer.

Lagrange quitted his native country, never to return. He took up his abode at Berlin on the 6th of November, 1766, and here he remained for the next twenty years.

In 1774 he won the prize of the Academy of Sciences for a powerful but unsuccessful attempt to explain that secular acceleration of the moon's mean motion which was discovered by Halley. In the same year appeared among the Memoirs of the Academy of Sciences the first of Lagrange's works on the stability of the solar system.

When the existence of perturbations, such as the long inequalities of Jupiter and Saturn, was proved, it naturally became a most interesting question whether, in the lapse of ages, their accumulation would result in the destruction of the solar system, either by its dispersion or by the falling of the bodies which compose it into the sun. In the Memoir of 1774, Lagrange proved that, in the case of two planets perturbing each other, the variation of the motion of their nodes and of the inclinations of the planes of their orbits oscillate between certain extreme values which are not very far apart. In 1776 Laplace extended this result to the variation of the eccentricities and the motion of the perihelia of the planets, in a memoir which was crowned by the Academy of Sciences.

In the same year Lagrange proved, in a paper which appeared among the Memoirs of the Academy of Sciences of Berlin, that the mutual perturbations of two planets could never produce any slow continuous, or "secular," variation in the magnitude of their mean distances from the sun. And in 1782, in an elaborate investigation which appeared among the Memoirs of the same body, he extended all these results to the case of all the planets mutually perturbing one another. In this paper he found that the periodic inequalities depended on the configuration of the planets, and the secular inequalities on their masses and orbits.

In these investigations it was necessary to know the relative magnitudes of the masses of the planets and the sun. In the case of those planets which have satellites, this is an easy thing to find; for when it is shown that the kind of perturbations the satellites suffer can be explained by the perturbing influence of the sun, the amount of those perturbations will depend upon the relative distances and masses of the planet and the sun; and since we can observe the magnitude of the perturbations, we can find the relative masses. In this way the relative masses of the earth, Jupiter, and Saturn were found; and then, knowing their magnitudes, it was ascertained that their densities vary inversely as their distances from the sun. In his investigations, Lagrange assumed this to be the case for all the planets; and, as there is no other ground than analogy for believing it, his great discovery of the stability of the solar system could not be considered to be rigorously proved.

In 1784, however, in probably the most remarkable memoir ever presented to a scientific society, Laplace demonstrated, by rigorous proof, the stability of the solar system, and solved the mystery of the long inequality of Jupiter and Saturn, and of the theory of Jupiter's satellites.

The stability of the solar system resulted from two theorems, which Laplace proved in this memoir. The first, relating to the oscillations in the form of the orbits, is as follows:—

"If the mass of each planet be multiplied by the square of the eccentricity, and this product by the square root of the mean distance, the sum of these quantities will always retain the same magnitude."

Now, it was found that this sum, calculated for a particular moment, was very small; hence, by the theorem, it will remain small; hence each term of the sum must remain small; hence the eccentricity cannot become considerable, since the masses and mean distances remain the same. The second theorem related to the positions of the orbits, and was as follows:—

"If the mass of each planet be multiplied by the square of the tangent of the orbits' inclination to a particular fixed plane, and this product by the square root of the mean distance, the sum of such quantities will continue invariable."

Hence, by similar reasoning, the planes of the orbits will not move far from their present position.

The investigation of the long inequalities of Jupiter and Saturn was reduced in possible range by a paper of Lagrange which appeared in 1783, in which he proved that these long inequalities in the motions of the planets could not depend on the secular variations of their orbits, but must somehow result from the periodic qualities of their motion.

In his great memoir of 1784, Laplace proved that they resulted from the fact that the periodic time of Jupiter is to that of Saturn nearly in the proportion of 2 to 5. It is obvious that these planets will perturb one another most when they are in conjunction; hence the effect of the perturbation on their motion at any point of their orbit will depend on the position of the line of conjunction. Suppose (Fig. 27) that Jupiter and Saturn are in conjunction at J_1 and S_1, then, when Saturn has made $\frac{2}{3}$ of a revolution, Jupiter has made $\frac{5}{3}$, or $1\frac{2}{3}$, of a revolution, and they will be again in conjunction at J_2 and S_2. Similarly, they will be again in conjunction at J_3 and S_3. There are, therefore, three equidistant points in their orbits at which they are in conjunction. But this is not exactly true. The proportions of the periodic times is not exactly 2 to 5, it is nearer 29 to 72; and therefore, when, after five revolutions, Jupiter is at J_1 again, Saturn will have passed S_1 a little, and Jupiter will have to go on a little way before conjunction takes place; hence all these points of conjunction slowly progress, and in 929 years $J_1 S_1$ will be in the position of

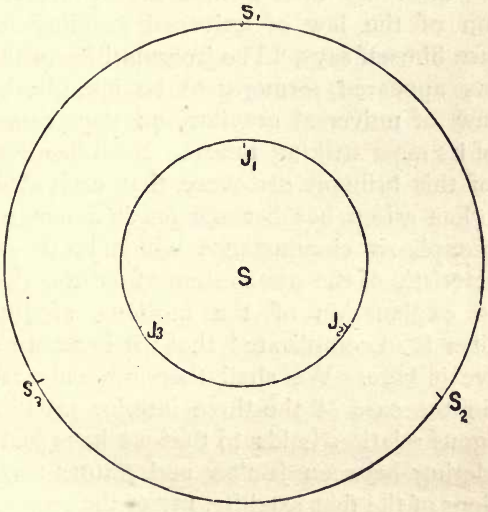

FIG. 27

$J_3 S_3$, and the three points of conjunction will be the same.

Laplace proved that this period of 929 years in the variation of the conjunctions would be that of an alternate acceleration and retardation of the mean motions of the planets, which he calculated to be the result of the progression of the points of greatest perturbation; and that the last moment at which the acceleration changed into retardation, and *vice versa*, took place in 1560; which accounts for the fact that Halley deduced an opposite result from the comparison of ancient observation to that obtained by comparing more modern ones.

This discovery then formed a still further verification of the law of universal gravitation. As Laplace himself says, "The irregularities of the two planets appeared formerly to be inexplicable by the law of universal gravitation; they now form one of its most striking proofs. Such has been the fate of this brilliant discovery, that each difficulty which has arisen has become for it a new subject of triumph—a circumstance which is the surest characteristic of the true system of nature." *

The explanation of the motions of Jupiter's satellites is so complicated that we have not space to give it here. We shall, therefore, only remark that in the case of the three interior satellites an analogous relation holds to that we have just been considering between Jupiter and Saturn; 275 revolutions of the first satellite, 137 of the second, and 68 of the third, are completed in almost exactly the same time, and since 275 exceeds the double of 137 by 1, and 137 exceeds the double of 68 by 1, the lines of conjunction of the first and second and the second and third regress at the same rate. This is the secret of their peculiar perturbations.

* Quoted in Grant's "History of Physical Astronomy," p. 60, from "Méchanique Celeste," tom. v. p. 324.

We must, however, give a more detailed account of another remarkable paper of Laplace, which appeared in the Memoirs of the Academy of Sciences for 1787, and which explained that secular acceleration of the moon's mean motion, which was first discovered by Halley.

Halley first pointed out the fact in 1693, but it was not regarded as established until 1749, when Dunthorne communicated a memoir to the Royal Society, in which he discussed all recorded observations likely to throw light upon the subject. He calculated, by the modern tables, the moment of an eclipse of the moon observed at Babylon in 721 B.C., another observed at Alexandria in the year 201 A.D., a solar eclipse observed by Theon in A.D. 364, and two others observed by Ibyn Jounis, at Cairo, towards the end of the tenth century. In all these cases the observed moment was later than the calculated moment, and the error was greater the more ancient the eclipse. The acceleration was in this way ascertained to be about ten seconds in a century, and the error of the earliest eclipse was only about two hours.

The explanation was attempted by several astronomers, but they all failed; and Euler went so far as to state at the end of a paper on the subject which gained the Academy prize for 1770, "There is not one of the equations about which any uncertainty prevails; and now it appears to be established by indisputable evidence, that the secular inequality in the moon's mean motion cannot be produced by the forces of gravitation."

Laplace at first tried to account for it by the retardation of the earth's rotation by the trade winds blowing on the mountains; but he proved that this effect could not be produced. He next supposed the existence of a medium pervading space which would cause the moon to be dragged in towards the earth and so to revolve quicker, but the want of independent evidence of this led him to reject this explanation. At last, however, in 1787, he found the secret. The researches of himself and Lagrange had demonstrated that the eccentricity of the earth's orbit oscillated between extreme values, and that for many centuries it had been diminishing, while the mean distance remained the same. The result of this was that the average distance of the earth and moon from the sun throughout the year had been for many centuries slowly increasing; and hence the perturbing influence of the sun had been diminishing, and consequently that enlargement of the moon's orbit which is part of her "variation," had been decreasing, and therefore, by Kepler's third law, her velocity had been slowly increasing.

Laplace calculated that the moon would be accelerated from this cause by about 10″ a century; his result therefore agreed exactly with observation. We shall see,* however, that in our own time a mistake has been discovered in his reasoning,

* *Vide* p. 325.

and that in reality the acceleration of the moon from this cause is only about half what he calculated. But still an acceleration of the moon's motion due to this cause does really exist, and was discovered by Laplace in the way we have described.

This is one of the most striking of astronomical discoveries, linking, as it does, the remotest periods of scientific research. Little did those painstaking old Magi, carefully recording their observations from the Tower of Belus while Hezekiah was reigning in Judah, think that, more than twenty-five centuries later, their thorough work would afford the most absolute verification of the true theory of the universe.

We must pass over the rest of Laplace's work with a mere mention of its existence. He demonstrated that the oscillations of the ocean could have no influence on the precession of the equinoxes; that the oscillations of the ocean were in a state of stable equilibrium, and that therefore no great wave would ever flood the land; that the ring of Saturn cannot be solid, or it would not be in a state of stable equilibrium, and that therefore it must consist of a multitude of small particles moving about freely among themselves; but these and other researches of Lagrange and Laplace we have no space to explain here.

These two mathematicians lived through the stirring times of the French Revolution, and were not wholly inactive in public affairs. On the death of Frederick the Great, Lagrange determined if possible to leave Berlin. He received invitations from several of the courts of Italy, but he refused them all. At last Mirabeau, being on a visit to Berlin, met him, and invited him to France. This invitation was eagerly accepted, and in 1787 he took up his abode in Paris.

While he was producing his wonderful series of papers at Berlin, he was secretly preparing his greatest work the "Méchanique Analytique," which was finished in 1786, and was published at Paris in 1788. About this time and for some years after, he seems to have taken a great disgust at mathematics, and to have devoted his attention to other studies—the history of religions, of ancient music, etc. His services were, however, demanded by the Republic for various scientific purposes. The National Assembly appointed him on the committee for the introduction of the decimal system of measures; and in March, 1792, he was made one of the three administrators of the French Mint.

On the 16th of October, 1793, the decree for the expulsion of all aliens from France was promulgated. Lagrange, however, was excepted, in in order to work at the theory of projectiles for the government. This, however, seemed for some time by no means a happy permission; for in the days of the "Terror," his life was in danger. In that mad time men of science were not spared. Lavoissier fell a victim, and soon after, Bailly, an astronomer, whose work we have not had space to notice. Bailly had been Mayor of Paris, and in the early days of the Revolution was one of the most popular of the popular leaders of France; but a far-fetched charge was brought against him, and he was condemned. His last moments were typical of all his intrepid life. As he was being led to the guillotine, a bystander shouted at him, "Bailly, you tremble!" "My friend, 'tis with cold," quietly replied the astronomer, and passed on to execution.

The terrible days passed away, and Lagrange was still alive. In the reconstruction of society which followed, he was appointed head of the professors who were to reorganize the Polytechnic School. From this time his love of mathematics returned, and some of his greatest works were written in the years that followed. On the foundation of the Institute of France out of the ruins of the old Academy, Lagrange's name stood first on the roll; and it was he who received General Bonaparte, as a member of the Institute, on his return from the conquest of Italy.

During the empire, honours gathered thick upon him. Member of the Senate, Grand Officer of the Legion of Honour, Count of the Empire,—these dignities were successively conferred upon him. At last, in 1812, he determined to bring out second editions of his "Méchanique Analytique" and his "Théorie des Fonctions Analytiques." The severe work thus undertaken was beyond the strength of the old man, and it threw him into a fever. With the calmness which he displayed throughout his life, he carefully watched and recorded the phenomena of this, his last illness; and on the 10th of April, 1813, he died.

It had been well for Laplace's reputation if his career had ended at the same time. Whatever admiration we are bound to feel for the intellectual powers of the author of "the Almagest of modern times," we cannot feel any great respect for the man. The public honours of Lagrange came upon him naturally and unsought. It was not so with Laplace. He sought public honours, and that, not from belief in a particular policy, but, it seems, from vulgar ambition.

In 1796, he was one of the deputation that waited on the Council of Five Hundred, to declare their inextinguishable hatred to royalty. About the same time he proposed to the Institute that they should make an annual presentation of their labours to the representatives of the people. The resolution was carried, and he himself appeared at the bar of the assembly, at the head of the savants, and made, on their behalf, a pompous oration in eulogy of the Revolution.

The consulate found him equally submissive. The third volume of the "Mécanique Cèleste," whose date, "An. XI.—1802," suggests the transition of his opinions, is dedicated to the "Citizen Premier Consul," whom in the dedicatory letter he calls "Héros pacificateur de l'Europe." For six weeks, he was Minister of the Interior, under the First Consul; but Napoleon in the St. Helena Memoirs, describes him as a man "incapable of seeing anything but the infinitely little." In December, 1799, he obtained a seat in the "Sénat Conservateur," and he was successively Vice-President, and Chancellor of that body, in 1803. Under the empire he received various honours, and yet he was among the first to welcome back the Bourbons. The only piece of honesty we can find in his public career is his refusal to attend at the Tuilleries, during the "hundred days." If this represents his genuine opinions, one cannot have much respect for a consistency that revealed itself in attachment to a despotism that was not more guilty than contemptible. He was a member of the Legitimist Chamber, and was the sole member of the Academy who refused to sign their protest to the Government in favour of the freedom of the press; and he who once, in the name of the Institute, eulogized the Revolution before the Assembly, gave, as the reason for his present conduct, that the Academy ought not to meddle with political matters.

As might be expected, during his latter years, the old man was extremely unpopular; but in his last illness the feverish excitements of public affairs were forgotten, and his mind turned solely to the real work of his life. He was constantly talking of great plans of astronomical research which he would carry out when he got well; but these plans were not to be completed "here down." On the 5th of March, 1827, he died.

We have not been able to give in this chapter any adequate idea of the vast extent, and the importance to science, of the labours of the great French mathematicians. But the effect of their work on men's minds has not been wholly good. It is against them that the attacks which are too frequently made upon scientific education are really directed. It has been pleasant to record the simple faith of the old heroes of science: with the exception of Euler, none of those whose work we have been considering in this chapter had any of this. Yet let us not be betrayed into thinking evil of them therefor. The foundations of faith must be laid deep in truth. Even the greatest minds are inevitably influenced more by daily experience than by reason; and all the daily experience of Christianity that these men had was the tyranny of "the Church," and the debaucheries of "The Most Christian King." Placid acquiescence in current belief is easy and cheap, but the seeds of faith lie in the strong desire to find the truth, though the truth turn out to be misery; and there is promise of truer knowledge in the thought which, just a century after the death of Newton, Laplace, second to him alone, in that small and sacred band who have enlarged the bounds of astronomical knowledge, expressed on his death-bed in nearly the same words, "Ce que nous savons est peu de choses; ce que nous ignorons est immense."

CHAPTER X.

ON HERSCHEL AND THE DISTRIBUTION OF THE STARS.

WE have said that English astronomy died, after the death of Bradley, until the days of living men; still, during that period one piece of experimental work of some importance was executed which deserves mention. This related to the determination of the masses of the heavenly bodies. We have hinted, in the last chapter, how their relative masses might be found, by weighing them in turn against the sun. If, then, the mass of any one of them could be found, the masses of all the others would be known.

Two determinations were made in Britain, at the end of last century, of the mass of the earth. The first of them was made by Nevil Maskelyne (born London, October 6, 1732; died February 9, 1811), who was Astronomer Royal from 1765 to 1811. He saw that, if the law of gravitation be true, a pendulum hanging in front of a large mass ought to be drawn sideways by it a little out of the perpendicular, and the amount by which it would be drawn aside would depend on the ratio of the attracting mass to the mass of the earth. The experiment was tried by the sides of the Schehallien Mountain, in Perthshire, a steep-sided, lofty ridge, running east and west. Knowing the size of the earth, it was calculated that two plumb-lines, one on the north, the other on the south side, ought to be inclined to one another, owing to the curvature of the earth, by an angle of 41″; it was found by observation of the stars that they were actually inclined at an angle of 53″, the attraction of the mountain on the northern plumb southwards, and on the southern one northwards, together producing this additional inclination of 12″. From this fact, the ratio of the mass of the earth to that of the mountain could be found. The mountain was then carefully surveyed in order to find its volume, and specimens of its rock were weighed in order to find its density; and so its mass was found, and then the mass of the earth was known.

This, however, is but a rough method. A more accurate one was adopted by Henry Cavendish (born October 10, 1731, died February 24, 1810). This method, which is usually called after his name, the "Cavendish Experiment," was first suggested by the Rev. John Michell, but he never lived to carry it out. It was then taken up by Cavendish, and the most trustworthy experiments were made afterwards by Mr. Francis Baily. A complete explanation of it would be too long to give here. The essential principle of it, however, is that two small balls are attached to the ends of a light rod, which is suspended by its middle point from a wire; two large leaden balls are then brought near the small ones, and attract them so as to twist the wire, which resists twisting with a force proportional to the twist. Knowing the masses of the balls, and the distances between their centres, and the force exerted by the wire, we obtain an equation between the masses, distances, and force exerted. The same equation must hold between the masses of the earth and any ball it attracts, the distance of their centres apart and the force between them. We know in this case the mass of the ball, the distance, and the force; therefore the mass of the earth can be found. And then, as we have already pointed out, the masses of all the other bodies of the solar system are known.

Besides these two experiments, there was going on in England, during the days of the great French mathematicians, astronomical work of the very highest order; but it was not executed by an Englishman. Since we considered the life of Kepler, who in his "laws" laid the foundations, we have been solely concerned with the rise, development, and verification of the true theory of physical astronomy. We left formal astronomy, with the first law of Kepler, when the actual motions and positions of the bodies of the solar system had been determined; while nothing had been ascertained about the positions of the stars, except that they were at an enormous distance from the solar system—a distance beside which the dimensions of that system sink into insignificance. From the time of Kepler to that of which we are now speaking, some work in formal astronomy had been done, and the labours of William Herschel, which are the subject of this chapter, were wholly concerned with it.

We have seen that in the Copernikan system the old notion of a sphere holding the stars still survived, but that sphere was fixed. But the discoveries of Galileo shook men's complacent confidence in their old notions. His telescope showed that the universe was very unlike what men had supposed it to be. He found that the Milky Way was composed of vast clusters of myriads of stars; that there were five hundred stars in the constellation of Orion alone; that there were thirty-six in the Pleiades, and so on. From this time forth the idea of a fixed sphere seems to have been dropped, and the notion that the stars were distributed unevenly throughout space, to have been adopted. But with that exception, nothing seems to have been discovered about the universe of stars. The only additions to formal astronomy were further observations on the solar system. Thus Galileo and the Jesuit Scheiner discovered by his spots that the sun rotated on an axis; Dominic Cassini (born Perinoldo, 1625; died Geneva, 1712) discovered, by observing marks on their surfaces, that Jupiter and Mars also rotated, and was the first to notice a hazy light surrounding the sun and elongated in the direction of his equator, which is now called the zodiacal light. He also discovered that Saturn's ring is double, consisting of an inner and an outer one, with a space between. But this, with the successive discoveries of the satellites of Jupiter and Saturn, was all that was done in formal astronomy between the days of Kepler and Herschel.

In Herschel we meet once more with an almost ideal character, and the story of his life is as pleasant and attractive as that of the ancient pioneers of knowledge.

He was born at Hanover, in November, 1738, and was of an old Protestant family. His father was a musician, and, at the time of the birth of the future astronomer, was oboeist in the band of the guards at Hanover. His mother, though kind and affectionate to her husband and children, cannot be credited with any influence over the future greatness of her son, for she was a person of little education, who believed that all the sorrows of the family had come from too much learning, and who consequently did her best to ensure that at least her daughters should not suffer from this misfortune. Accordingly she prevented them from learning the French and dancing and other accomplishments which at that time made up the education of girls, and taught them only cooking and sewing and the care of the household.

She had ten children, of whom six grew up. Of these three only are interesting to us—Frederic William, the fourth child; John Alexander, the sixth; and Carolina Lucretia, the eighth.

The sons were better treated than the daughters. They were all sent to the garrison school in Hanover until they were fourteen, and the father took a special care of the education of each of them. They were all intended for musicians. Some account of their life after they left school has been given by Carolina Herschel. She says, "My brothers were often introduced as solo performers and assistants in the orchestra of the court, and I remember that I was frequently prevented from going to sleep, by the lively criticism on music, on coming from a concert, or by conversations on philosophical subjects. . . . Often I would keep myself awake that I might listen to their animating remarks, for it made me so happy to see them so happy. But generally their conversation would branch out on philosophical subjects, when my brother William and my father often argued with such warmth that my mother's interference became necessary when the names of Leibnitz, Newton, and Euler sounded rather too loud for the repose of her little ones."

William was in these days fond of making instruments to be used in the study of astronomy, and he was helped by his father and brother Alexander.

In 1755 the Hanoverian guards were ordered to England, and the father, William, and Jacob, the eldest brother, went in the band, returning at the end of a year. In 1757, William seems to have served with the guard in that campaign in which the Duke of Cumberland was beaten by Marshal D'Estrées, and left the French generals free to turn all their forces against Frederic the Great. But his health was so delicate that it was resolved to remove him from the army and send him to England. This "removal" seems to have been, technically at least, a desertion, for years afterwards, when he was first presented at court, after his discovery of Uranus, George III. handed him a pardon, the need of which had been probably long forgotten.

Herschel was now nineteen years old, in England, having to earn his bread as best he could; and, as his biographer says, "All his equipment was 'good linen and clothing,' a knowledge of French, Latin, and English, some skill in playing the violin, the organ, and the oboe, and an 'uncommon precipitancy' in doing what there was to be done." *

We first meet with him as organizing the music of a corps of the militia of Durham. In 1760, Dr. Miller, then a noted organist, heard of Herschel while dining with the officers of the militia, was introduced to him, and was so attracted by him, that he asked him to come and live with him, saying that he was worthy of far higher work than

* "Sir William Herschel, His Life and Works," by Edward J. Holden, p. 17.

he was then engaged in. Dr. Miller introduced him to Cropley's concerts, where the first violin was soon assigned to him. From this time he became known as a musician, and soon after he was appointed organist to the parish church at Halifax. In 1766 he obtained a better appointment as oboeist at Bath, then the resort of the fashion and rank of the kingdom. Here as everywhere he soon made friends, and obtained a large number of pupils; and, what was an especial delight to him, he had access to excellent libraries. For the increase of business and his growing success in life only seemed to increase his thirst for knowledge; and during these years he mastered Italian, made some progress in Greek, and read a good deal of mathematics. Still, however, from the very first he seems to have held, as the great ambition of his life, that he might one day be able to "see for himself" the phenomena of astronomy.

So his life went on until 1772, when a great change came over it; one, however, which would not have been obvious to an outsider, and one, the import of which even he himself probably did not fully realize. In that year his sister Carolina came to live with him at Bath. Ever since she had felt "so happy to see" her brothers "so happy," she had been growing up not much noticed by her family, but lavishing her love on them, and especially on her absent brother. When her father died in 1767, the whole wealth of her affection was bestowed on William; and from the time when, to her inexpressible delight, he carried her to Bath, she devoted her whole life to him with unwavering loyalty; and it is not too much to say that without her help the greatest of his discoveries would never have been made.

The life at first was hard. Carolina had to learn English, and to educate her voice that she might become a soloist in her brother's concerts. The education she had received from her mother fitted her little for the life she was to lead; and she says of this period, "As I was to take a part the next year in the oratorios, I had, for a whole twelvemonth, two lessons per week from Miss Fleming, the celebrated dancing mistress, to drill me for a gentlewoman (God knows how she succeeded!)" Through all this she suffered from home sickness and melancholy, yet she struggled on bravely, and soon better things were in store for her.

William was now prospering, and he determined to buy a telescope to "see for himself" the facts he had read of. But he found no large telescopes were ever made; no reflectors above a few inches diameter. In this perplexity he determined to make one for himself. He bought some old tools for the purpose, and set to work. His first telescope was completed in 1774, and he began to study the stars. Next year at odd moments, and even "between the acts of the opera," Herschel made his first review of the heavens, which consisted of a careful examination of all the stars of the first four magnitudes.

William, Carolina, and Alexander, who was now at Bath, assisting in the musical work, were now in a larger house, where there was "more room for workshops," and the success of the first attempt at telescope making inspired them to further efforts. In 1775, a seven-foot reflector was completed; in 1777, a ten-foot; and in 1778, a "very good" ten-foot took its place. All the reflectors were made by the hands of the two brothers and Carolina.

Another change of residence was made, and the observations went on with increasing vigour. The mountains in the moon were now the object of Herschel's investigations. "In 1779, 1780, and 1781," he says, "I measured the heights of about one hundred mountains of the moon by three different methods." The plan usually adopted was to measure the length of their shadows; then, knowing the position of the sun relative to the moon, the relative magnitudes of the height of the mountain above the surrounding plain, and the length of the shadow, would be known.

While engaged upon this work Herschel made the acquaintance of Dr. William Watson, a Fellow of the Royal Society, and through him he became a member of the Bath Philosophical Society, where he met kindred spirits. Among his earliest papers read before this society, were some on central forces, other than that of gravitation, which may be conceived to be concerned in the construction of the siderial universe. This was the first indication of his thoughts being turned to that particular branch of astronomy in which his future triumphs were made.

Through Dr. Watson, Herschel now began to communicate papers to the Royal Society. The first two of these appeared in the Philosophical Transactions for 1780, the third and fourth in 1781. The fourth is a very remarkable paper. It is entitled, "Account of a Comet." While observing on the night of Tuesday, March 13, 1781, he says, "In examining the small stars in the neighbourhood of H Geminorum, I perceived one that appeared visibly larger than the rest. Being struck with its uncommon appearance, I compared it to H Geminorum, and the small star in the quartile between Auriga and Gemini, and finding it so much larger than either of them, I suspected it to be a comet." This idea was so far confirmed that the "comet" was found to change its place among the stars. The astronomers of Europe were now engaged in finding its orbit, but no elongated ellipse would account for its motion. In this perplexity the distinctness of the new "comet" suggested that it was not a comet at all, but a planet; and finally Laplace (in January, 1783) determined this to be the case, by finding that its apparent motion could only be explained by supposing that it moved in a nearly circular orbit, at a distance from the sun about nineteen times that of the earth, and about twice that of Saturn. This was perhaps the most striking observation that had yet been made in astronomy. Since the discovery of the last of Cassini's satellites of Saturn, in 1684, no new object had been discovered in the sky; and now, at one stroke, the unprecedented discovery of a new primary planet had been made, and the limits of the solar system had been doubled.

The name of the new planet had to be now settled. Some naturally wished to call it Herschel; but analogy to the names of the other planets prevented this, and the remotest of the planets received the name of the father of the gods—Uranus. The discoverer himself wished to call it Georgium Sidus, and he was prompted to this not merely by conventional loyalty. George III. was a real benefactor to him. Shortly after the discovery of Uranus, Herschel was summoned to London to exhibit his telescope and its revelations to the court. The result of this visit was that the king bestowed on him a pension of £200 a year, and undertook to defray the cost of fresh instruments, if he would give up his musical work and devote himself to astronomy. The offer was eagerly accepted, and the astronomer and his sister removed to Datchet on the 1st of August, 1782.

Herschel's name was now widely known. He received the Copley medal of the Royal Society in 1781, for his "discovery of a new and singular star," and on the 6th of December of the same year he was elected a Fellow. But his work hitherto was nothing to what he intended. While he was in London exhibiting his instruments to the king, he wrote to Carolina, "Among opticians and astronomers nothing now is talked of but what they call my discoveries. Alas! this shows how far they are behind, when such trifles as I have seen and done are called great. Let me but get at it again! I will make such telescopes, and see such things—that is, I will endeavour to do so."

The enthusiasm with which he carried out these endeavours may be seen in Carolina's description of her attendance on her brother. "By way of keeping him alive," she says, "I was constantly obliged to feed him by putting the victuals, by bits, into his mouth. This was once the case when, in order to finish a seven-foot mirror, he had not taken his hands from it for sixteen hours together. In general, he was never unemployed at meals, but was always at those times contriving or making drawings of whatever came in his mind."

The largest telescope that was completed at Bath was a twenty-foot—an instrument of unprecedented power; but a larger one was even then contemplated, and the mirror for a thirty-foot was cast, after several attempts; at the first it cracked in cooling; at the second the furnace broke; ultimately it was made.

The pension of £200 was not enough for the scientific needs of the Herschels, and at first it seemed likely that they would have to waste time in making telescopes for money. This difficulty was soon got over by two events. The chief men of science in the kingdom persuaded George III. to give £2000 to defray the expenses of a large telescope for the use of the astronomer himself, and in 1783, on the 8th of May, Herschel married a lady who seems to have been worthy of him, entirely devoted to his pursuits, and possessed of a fortune.

With the king's bounty, the celebrated telescope of forty feet focal length, and four feet diameter, was commenced and completed in 1787.

In 1785 the family removed to a larger and better house, Clay Hall, in old Windsor, and next year to Slough, where Herschel remained for the rest of his life. One little sentence in Carolina's diary reveals to us the almost fierce earnestness of the great observer. "The last night at Clay Hall was spent in sweeping till daylight, and by the next evening the telescope stood ready for observation at Slough."

We must now give some account of the discoveries made in this new Uraniburg.

With the exception of his researches on the height of the mountains of the moon, Herschel does not seem to have paid much attention to our satellite. The other bodies of the solar system interested him somewhat more; for, besides the discovery of Uranus, he discovered two more satellites to Saturn—the sixth and seventh—in 1789. In 1792 he showed that the most distant satellite of Saturn rotates at the same rate as he revolves, like our moon; and in January, 1787, he found two satellites to Uranus. His theories as to the nature of the sun have been since found to be, for the most part, erroneous, and therefore we cannot stay to consider them here. One point must, however, be noticed as illustrating his extraordinary fertility of resources. He noticed a periodicity in the appearance of sun spots; he wanted to find whether there was a corresponding periodicity in the heat received from the sun. For this meteorological observations for many years were needed. None such existed; but Herschel saw that the greater the heat from the sun, the better would be the season, the greater the produce; and hence he was led to employ the price of wheat as an indication of the amount of heat received from the sun—a criterion which has, since his time, been much used.

During his life a further discovery was added to our knowledge of the solar system, under somewhat curious circumstances. Johann Elert Bode (born Hamburg, January 19, 1747; died Berlin, November 23, 1826) pointed out that the difference between the distances of consecutive planets from the sun doubles for every consecutive two, or, that if we take the series of numbers, 0, 3, 3 × 2, 3 × 2 × 2, 3 × 2 × 2 × 2, and so on, multiplying each by two to get the next, that is, the series 0, 3, 6, 12, 24, 48, 96, 192, and add 4 to each, so as to get the series 4, 7, 10, 16, 28, 52, 100, 196, these numbers will represent the relative distances of the planets, if we omit the fifth number, which would represent a planet whose orbit would lie between that of Mars and Jupiter.

This fact suggested that a planet hitherto undiscovered might lie between the orbits of Mars and Jupiter; and an association of twenty-four German astronomers was formed to search for it. None of them found it; but on the first day of this century, Piazzi, an Italian astronomer, noticed a very small star which, on the following evening, had changed its place. Careful observations showed that it was the missing planet. Next year another was found, moving in nearly the same orbit, and since then about a hundred and fifty have been discovered. All these "Asteroids," as they are called, are extremely small, and move in nearly the same orbit, hence it was for a long time supposed that they were fragments of an exploded planet; but we shall find a better explanation of their phenomena in the next chapter.

The great work of Herschel related, however, not to our system, but to the universe of stars. The first question that naturally comes up is, does the law of gravitation hold good for the stars as well as the planets? If it does, they must be moving, though, owing to their great distances from one another, extremely slowly. By comparison of observations with old catalogues of the stars, Maskelyne determined that several of them were moving, and Herschel found still more.

But for the same reason the sun himself, with his system of planets, must be moving through space; and, if so, the apparent distance of the stars apart will become greater, in that direction towards which he is moving, and less, in the opposite direction. This will be complicated with the actual motion of the stars themselves; but still, on the whole, the heavens will appear, so to speak, to open up in front of the solar system and close behind it. Herschel, by this principle, was able to show by his observations, that the sun is moving towards the constellation of Hercules.

So far the law of gravitation is satisfied; but we have a much more particular test than this. Herschel discovered more than two hundred stars, which look like single stars to the naked eye, to be really double when seen through a telescope; the chances would be practically infinite against all these pairs of stars being sets of two quite separate stars, happening to be in the same line with the solar system, and, probably therefore, they form systems of two suns close together. Since Herschel's time, this has been found to be the case, for the double stars are observed to describe orbits about each other, and the law of gravitation holds good between them, for their motions are observed to obey Kepler's laws.

The great object, however, which Herschel kept before him, was the determination of the distribution of the stars. He assumed as the basis of his theory that the stars were, on the average, uniformly distributed throughout the space they occupy, and were, on the average, of equal brightness. It is important to understand the meaning of this. He did not assume that each of the stars was at the same distance from the nearest ones to it, and all of the same brightness; but, knowing that his twenty-foot telescope could bring within his sight more than five million stars, he assumed that, say, ten thousand seen in any one small patch of the sky, would not be, on the average, of very different brightness, or much more or less closely clustered, than ten thousand in any other patch of the sky. By means of this principle he was able to determine the shape of the space through which the stars are distributed; for wherever there seems to be a great clustering of stars in the sky, it must be, according to this principle, that we are looking through a greater thickness of stars, so to speak, than in any other direction, in which the stars do not appear so thickly clustered; just as in a forest, where the trees are young and old, big and little, and not all planted at exactly even distances apart, but where no one part of the forest has many more big trees in it, or has its trees much more crowded together than any other part, a person in the forest near its edge will see the trees sparsely scattered in the direction in which the edge of the forest is near to him, and closely clustered in the direction in which it is far off.

Now, in this case of the forest, the near trees will look larger than those more remote, and so the trees that appear large from their nearness will be evenly distributed all round the observer, and the clustering of trees in the direction in which the edge of the forest is far off, will be caused by a clustering of trees which, from their distance, look small. Herschel's assumption is to some extent verified by observing that this is exactly the case with the stars. Stars of the first four magnitudes are distributed nearly evenly over the sky, and the clustering in the direction of the Milky Way is caused solely by very minute stars.

Herschel, to use his own expression, "gauged" the heavens, by pointing his telescope in 863 directions, as nearly as possible evenly inclined to one another, and then counting all the stars he could see in its field of view in each direction. All the stars he could see in each direction existed in a cone of space, whose base was that part of the surface of the space containing the stellar universe which filled his field of view, and whose apex was his eye, or the solar system. The volume of different cones of this kind being proportional to the cubes of the distances of the bases from the solar system, he found the shape of the stellar universe by drawing 863 lines from a point which represents the solar system, in directions which represent the 863 directions in which he pointed his telescope, and to lengths proportional to the cube root of the number of stars he saw in the direction corresponding to each line. The ends of these lines were points on a surface of the same shape as that occupied by the stellar universe. Later on he somewhat modified this method, and adopted one which we have not space here to explain, but which proceeded on the lines of that which we have just described. In this way he found that the space occupied by the stellar universe resembled in form an oval grindstone with a cleft in its edge; so that, looked at one way, it would look oval, looked at edgeways, it would appear to be, roughly speaking, of the shape represented in Fig. 28.

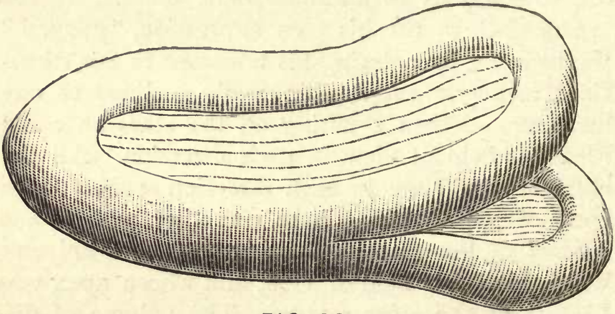

FIG. 28.

Since Herschel's day, Bessel, Henderson, and Maclean have found two stars to possess a sensible parallax, the diameter of the earth's orbit being the base; that of 61 Cygni being about $\frac{1}{3}''$, and of *a* Centauri, about $1''$.* The latter, which is the nearest known star, must therefore be distant twenty billions of miles from the solar system. On Herschel's principles, as to the distribution of the stars, it follows that the length of the stellar universe cannot be less than twenty thousand billions of miles. The mind fails altogether to grasp these magnitudes; but some idea of them

* There are seventeen other stars, for which a parallax has been less certainly found.

may be gained by considering that light takes eight minutes to traverse the ninety-one millions of miles from the sun to us, and that at this rate it would take three years to come to us from the nearest star, and three thousand years to traverse the length of the cluster which we have called the stellar universe.

The results we have just obtained would be meaningless if every additional telescopic power opened up to our view more stars ; but this is not the case. Besides the stars which Herschel saw with his twenty-foot telescope, he could see flakes of faint light, resembling the Milky Way as seen by the naked eye, and these were more numerous in that direction where the stellar universe is thinnest, and where, therefore, fewest stars were to be seen. The great forty-foot telescope "resolved" some of these into distinct clusters of stars, and hence they must be stellar universes altogether outside that whose shape we have been considering. Knowing roughly, by methods already explained, how far the eye can see into space, and knowing how many times further the telescope that will just resolve these flakes of faint light, or "nebulæ" as they are called, can penetrate, it can be estimated that the light coming to us from some of these far-off universes cannot take less than than two millions of years to reach us. In observing these vast systems we are therefore, as Herschel said, "studying their ancient history."

It might be supposed, since every additional telescopic power since Herschel's days has resolved more of these nebulæ, that, in reality, they all consist of clusters of stars. There were, however, two objections to this: the great nebula in Orion, one of the largest in the sky, could not be resolved; it seemed improbable that so large a mass was really a vast universe, as far off as any the telescope reveals to us, and therefore far vaster than any other we know of. This objection was, however, afterwards removed, for the great reflector of Lord Rosse did at last resolve it.

The other reason for believing some nebulæ to be really of a gaseous nature, was the phenomenon of nebulous stars. Herschel observed a multitude of nebulæ to be of a perfectly circular form, and none of these could be resolved. This is just the form they would present if they were really spherical, and the spherical shape is exactly what a gas would assume under the mutual gravitation of its parts. Most of these bodies were observed to have a brighter nucleus concentric with them; and these nuclei were observed in every stage of brightness. But the brighter they are the smaller they are; and in some cases a bright distinct star exists at the centre. In this latter case, it seems probable that the nebula is a mass of gas surrounding a star belonging to our universe. But we shall return to this subject in the next chapter.

The labour involved in collecting so vast a mass of observations, as was needed to support these theories, was probably far greater than has ever been undertaken by any other observer. Throughout, Sir William Herschel was assisted by his sister, who was trained by him to be an observer equal to himself. In the course of her work with him she herself discovered no less than eight new comets. And yet their life was by no means austere. Music, their early business, was throughout life their great delight.

Wealth and honours came upon the great astronomer, but they in no way affected him. A beautiful picture of him has been left us by the poet Campbell. Writing to a friend on September 15, 1813, he says, "I wish you had been with me the day before yesterday, when you would have joined me, I am sure, deeply in admiring a great, simple, good old man—Dr. Herschel. . . . His simplicity, his kindness, his anecdotes, his readiness to explain—and make perfectly conspicuous, too—his own sublime conceptions of the universe, are indescribably charming. He is seventy-six, but fresh and stout; and there he sat, nearest the door at his friend's house, alternately smiling at a joke, or contentedly sitting without share or notice in the conversation. Any train of conversation he follows implicitly; anything you ask he labours with a sort of boyish earnestness to explain. . . . Speaking of himself, he said, with a modesty of manner which quite overcame me, when taken together with the greatness of the assertion, 'I have looked further into space than ever human being did before me. I have observed stars, of which the light, it can be proved, must take two millions of years to reach this earth.'"

At that time he was still in full vigour, but he soon began to grow feebler; still he worked on, and even in 1822 communicated a paper to the Royal Astronomical Society. His work was, however, by this time done, and on the 25th of August in that year, the old hero passed away.

CHAPTER XI.

ON MODERN ASTRONOMY.

WE have now brought our sketch of the lives and labours of the astronomers down to our own time, and we must give some account of the present state of the science. This account must necessarily be brief; and we must pass over, with a mere mention, the ingenious speculations of Struve; the labours of Bessel; of Sir John Herschel, whose observations almost rivalled those of his father, and whose mathematical powers far surpassed his; of Sir George Airy, who, amongst other important investigations, has discovered a long inequality in the motion of the earth and Venus exactly analagous to that of Jupiter and Saturn, and depending on the fact that eight times the periodic time of the earth is almost exactly equal to thirteen times that of Venus; of Professor Hansen, who has discovered and explained two new inequalities in the moon's motion; and of a host of other workers.

The most remarkable verification of the law of gravitation, and perhaps the hardest physical problem that has ever been solved, has been reserved for the days of living men, and must be more particularly noticed.

After the discovery of Uranus, and the approximate determination of its orbit by Laplace, it was found, by calculating its position at various moments, that it had been previously observed and catalogued as a star no less than nineteen times. These observations served to bring the determination of its motion to a great degree of accuracy. It was found that it suffered perturbations like the other planets, and these perturbations were accounted for on the theory of gravitation. As, however, more accurate observations were made, minute irregularities were found which defied explanation. Bouvard investigated this subject most carefully, and suggested as a possible explanation that there might exist a planet, hitherto undiscovered, to whose influence the perturbations were due. Dr. Hussey, in 1834, gave reasons for believing that, if such a planet existed, its orbit must be outside that of Uranus; but so uncertain was the reasoning hitherto brought to bear upon the question, that Hansen soon afterwards argued that it was necessary to assume the existence of two exterior planets.

In 1841 a young man, Mr. John Couch Adams, was studying as an undergraduate at St. John's College, Cambridge, and having read Professor

Airy's report to the British Association on the progress of astronomy in the nineteenth century, he entered in his note book, under date July 3 of that year, "Formed a design, in the beginning of this week, of investigating, as soon as possible after taking my degree, the irregularities in the motion of Uranus which are yet unaccounted for, in order to find whether they may be attributed to the action of an undiscovered planet beyond it, and if possible, thence to determine approximately the elements of its orbit, etc., which would probably lead to its discovery."

In January, 1843, Mr. Adams graduated as Senior Wrangler, and immediately set to work on the theory of Uranus. The extreme difficulty of the problem may to some extent be appreciated when we remember that it is the inverse of the ordinary problem of perturbations. Given the positions and movements of the planets, the great mathematicians of last century had found it sufficiently difficult to calculate the perturbations; but given the perturbations, it was a far harder problem to find the mass and mean distance of the perturbing body, together with the eccentricity and plane of its orbit, the direction of its line of apses and the position, at a particular moment, of the planet in its orbit. Yet, in September, 1845, Mr. Adams had solved the problem, and sent the result to the Astronomer Royal, stating where he was to point his telescope on the 30th of September to find the planet.

Mr. Adams's investigation was based on the observed error in longitude of the motion of Uranus; the Astronomer Royal, before entering upon a search for the planet, asked that the theory should be verified, by applying it to the error in radius vector, which had lately become large. For some reason this verification was neglected, and the investigation, so far, was not published.

Nine months later, in June, 1846, the volume of the Comptes Rendus contained a paper by a French astronomer, M. le Verrier, on the theory of Uranus, in which its perturbations were explained by the attraction of a planet whose motion and mass were determined to be the same as those found by Mr. Adams. The attention of the practical astronomers of Europe was now roused to the importance of the subject, and a search was instituted for the new planet. Telescopes were pointed in the direction specified by the mathematicians, and careful maps were made of all the stars in its neighbourhood, in order to find which of them changed its position. In this way the new planet, which received the name of Neptune, was first seen by Professor Challis, at Cambridge, on the 4th of August, 1846, but was first recognized as a planet by Dr. Galle, at Berlin, on the 24th of September of that year, within 1° of the place assigned to it by Adams, and within ½° of that calculated by Le Verrier.

It is important to notice that the researches of Adams and Le Verrier were entirely independent, conducted by different methods, and each unknown to the other. The honour of the discovery must be divided evenly between them, for while Adams's result was obtained rather earlier, Le Verrier's was rather more accurate.

This discovery is perhaps the most striking in the whole history of science. Without leaving his desk the mathematician was able to direct the observer to point his telescope to such a spot in the sky, and there he would see a new planet. Before it was seen by the eye of man, Sir John Herschel declared to the British Association, "We see it as Columbus saw America from the shores of Spain. Its movements have been felt, trembling along the far-reaching line of our analysis, with a certainty hardly inferior to ocular demonstration."*

The discovery of Neptune completes the verification of the true theory of physical astronomy, and is an addition to our knowledge of formal astronomy. To this branch of the subject, we have only to add the discovery of the fifth satellite of Saturn, raising the whole number to eight, simultaneously made by Professor Bond and Mr. Lassell, on the 19th of September, 1848; the discovery of two more satellites to Uranus by Lassell in 1847; the discovery of a satellite to Neptune by Lassell in October, 1846; the late discovery of two satellites to Mars by Professor Asaph Hall, at Washington, in August, 1877, and the ingenious

* At Southampton, September 10, 1846.

methods by which M. Foucault enables us, so to speak, to see the earth rotating.

In 1851, M. Foucault pointed out that if a heavy spherical weight be suspended by a long thin wire, and then be set swinging as a pendulum, there is no reason why it should change the direction in which it swings; but that if the earth rotates, carrying the room, in which the pendulum swings, round with it, the position of the room will change with respect to the constant direction in which the pendulum swings; and to an observer in the room, who will be carried round with it, the direction in which the pendulum swings will appear to change. He made the experiment in the dome of the Panthéon, at Paris, and found that the pendulum apparently changed the direction of its swing, exactly as it ought to do if the earth rotated once a day.

His other method was by means of an instrument called a gyroscope; which is in fact, merely an accurately made top; a circular disc with a thick rim, and an axis through its centre. The ends of the axis are mounted in such a manner that it can change its direction freely. We have already pointed out that it is a consequence of the laws of motion, which can be verified by experiment, that such an instrument, if set rapidly rotating, will have a strong tendency to keep its axis of rotation in the same direction. It is found, however, on trial, that if the gyroscope be set to rotate so rapidly that it continues to rotate for a long while, the direction of the axis does appear to change. But if the axis be pointed at a particular star when the instrument is set rotating, it will continue to point at that star, always following it round in its apparent course; showing that the line joining the earth to the star is sensibly fixed, and that the apparent motion of the star must be produced by the rotation of the earth.

It will be well, in concluding our account of formal astronomy, to sum up the general results we have arrived at. Sir John Herschel has described a model of the solar system, in which we should represent the asteroids by the smallest objects convenient, grains of sand. He says, "choose any well levelled field or bowling-green. On it place a globe, two feet in diameter; this will represent the sun. Mercury will be represented by a grain of mustard seed, on the circumference of a circle 164 feet in diameter for its orbit; Venus, a pea, on a circle 284 feet in diameter; the earth also a pea, on a circle of 430 feet; Mars a rather large pin's head, on a circle of 654 feet; the asteroids, grains of sand, in orbits of from 1000 to 1200 feet; Jupiter, a moderate sized orange, in a circle nearly half a mile across; Saturn, a small orange in a circle of four-fifths of a mile; Uranus, a full-sized cherry or small plum, upon the circumference of a circle more than a mile and a half; and Neptune, a good sized plum, on a circle about two miles and a half in diameter. . . . To imitate the motions of the planets in the above-mentioned orbits, Mercury must describe its own diameter in 41 seconds;

Venus in 4 minutes 14 seconds; the Earth in 7 minutes; Mars in 4 minutes 48 seconds; Jupiter 2 hours 56 minutes; Saturn in 3 hours 13 minutes; Uranus in 2 hours 16 minutes; and Neptune in 3 hours 30 minutes." * Round most of the planets revolve satellites, in nearly circular orbits. Of these the Earth has one; Mars two; Jupiter four; Saturn eight besides a ring; Uranus certainly four; and Neptune certainly one. Owing to their great distances, it is difficult to observe the last two planets, and it is quite possible that they may have more satellites, hitherto undiscovered.

But outside the limits of the solar system, and surrounding it, there is a universe of stars, all of which probably resemble our sun, many of which probably surpass him in size and brilliancy, and situated at distances from him, and from one another, which are incomparably greater than the magnitudes of the solar system; so that if we were to take all the nearest fixed stars that surround our sun, and weave a web from star to star, the vast orbit of Neptune, more than five thousand million miles across, would be, within the space so enclosed, but as a finger ring in a hall a thousand feet high, a thousand feet long, and a thousand feet wide.

And of the stars, there are between five and six million distributed through a space shaped like Fig. 28, which constitutes our cluster. But outside this again, the telescope reveals other clusters separated from it by inconceivable distances—other

* "Outlines of Astronomy," § 526.

universes of suns, each probably surrounded by attendant planets and satellites.

This is a summary of the answer which has been found to the question, How are the heavenly bodies really distributed, and how do they really move to produce the complicated apparent distributions and motions which we observe? and the answer constitutes Formal Astronomy. But then follows the question, Why are they so distributed, and why do they so move? The answer to this constitutes Physical Astronomy, and we have partially discovered the answer to the question in the law of gravitation—but only partially. The logical consequence of the law of gravitation is, that the planets and satellites ought to move in ellipses, with the primary in a focus of each, according to Kepler's laws, and perturb one another in a certain way, all of which agrees with observation. But whether the ellipse should be elongated or round, what its plane should be, which way the planet should move in its orbit; of these the law of gravitation tells us nothing. These all depend in each case on the way the body was originally started in its course.

Now in all the bodies of the solar system, with the exception of the satellites of Uranus and Neptune, we observe a very remarkable similarity in these respects. The orbits of all the planets and satellites are very nearly circles, and nearly in the same plane. All the planets and satellites rotate on their axes, and revolve in their orbits in the same direction. This seems to point to some common cause of their original starting on their courses. Laplace calculated that the odds against this being *accidental*, against, that is, each body having had an entirely isolated and separate physical cause of its original motion, was many trillions to one. It is therefore practically certain that there must be some one physical cause of the original motions of all the bodies of the solar system; and Physical Astronomy is not complete until we have discovered what that cause is.

The latest astronomical work has had to do with this question, and therefore, though our knowledge of the subject is in an extremely elementary condition, it will be well to give some account of it here.

The following account is merely an expansion of the suggestions made by Laplace in his "Système du Monde;" but the reader must remember that the subject has never been examined by a rigorous mathematical analysis, and therefore does not stand in at all the same category of exactness with the rest of Physical Astronomy.

The explanation is suggested by the observed phenomena of Sir William Herschel's nebulous stars. We have pointed out that these are observed in every stage of condensation, from planetary nebula where there is no nucleus, to bright distinct stars surrounded by a faint haze. Let us consider how such stars might have arisen, and what ought to happen to them according to the laws of motion and the law of gravitation, as condensation continues.

Suppose that the whole of the space now occupied by our cluster was once filled with a nebulous gas of extreme tenuity. Let us further assume that this gas is not perfectly homogeneous, and not perfectly at rest. These latter assumptions add no complexity to the first assumption ; for whatever were the immediate physical cause of the gas being there, the odds are practically infinite that so large a mass of such extreme tenuity would neither be at rest, nor homogeneous. There must, therefore, be points in the mass, round which the gas is denser than the gas at a little distance away from these points.

Under the action of the gravitation of its parts, the whole mass, which we may call a nebula, would tend to assume a spherical form, but it would take an enormous time for this to take place; and long before that time had arrived, the portions of gas of greater density would have attracted all the gas in their neighbourhood to themselves, and thus the whole nebula would be broken up into separate portions of gas surrounding points which were before points of greatest density. Each of these portions, which now is a comparatively small and dense mass of gas, would, under the gravitation of its parts, assume a spherical form. Every part of it would be attracted towards its centre, and hence the heaviest or densest portions would take up their place at the centre. It might be that there were some parts of the gas of much greater tenuity than the rest; these would surround the rest, in the form of a spherical shell, and might remain in that condition without altering. In this case, we have only to consider the central nucleus; in all other cases we will continue to consider the whole spherical mass, with its densest parts at the centre.

Every part being attracted towards the centre, it follows that there will be a continuous pressure on every part, and hence, since gas is compressible, the whole may continue to condense.

So far, we have supposed the mass to be at rest; but we made the natural assumption that the gas which formed the original nebula was not at rest; therefore, when the portions of the spherical mass we are considering were torn away from the rest of the nebula and attracted together, they would have other motions, besides that due to the attraction which draws them together; if we regard the whole spherical mass thus formed as a material point, this material point will only remain at rest if the original motions of its parts exactly balance each other. But it would be very odd if they did exactly balance; it is much more likely that they would have a resultant, and if they have, the whole spherical mass will move through space, with a motion which is the resultant of the original motions of its parts.

But the spherical mass must not be regarded as a material point—it is a sphere, many thousand million miles across; and its parts are capable of independent motion within itself. Since it is practically certain that the directions of the original motions of the parts would not have passed through the centre of the sphere, it follows that there will be a constant movement of the gas in the sphere. The friction and viscosity of the gas, which increase as condensation continues, constantly tend to, and will ultimately, bring the whole spherical mass of gas to move as one piece about its centre of gravity, the motion of rotation it will then have, being the resultant of all the independent currents in the mass or motions of its parts that existed after it had assumed a spherical form. As the odds are again practically infinite against all these motions exactly balancing each other, we may be practically certain that such a motion of rotation will exist.

We have now got a spherical mass of gas rotating about an axis, and contracting in volume; our object is to discover what its history will be. Consider any small portion, A, of the gas near its surface and the forces which act on it. There is a force of repulsion between the particles of any gas, the cause of which it is not our present business to inquire, but which would, if it acted alone, cause the spherical mass to expand, keeping the same shape and its centre being at rest. The resultant, therefore, of all such forces acting upon A (Fig. 29), must act along C A, where C is the centre of the spherical mass. But, besides this, by the law of gravitation, all the parts of the sphere attract the small part at A, as if the whole mass were collected at C, its centre, and therefore, since A remains on the surface throughout the contraction of the sphere, this attractive force on A varies inversely as the square of the radius of the sphere. These are the

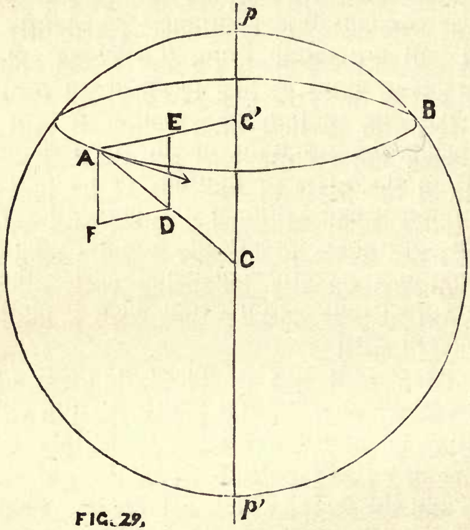

FIG. 29.

two forces on A; they act in the same line, but opposite directions, and the fact that the sphere contracts is due to the excess of the attractive force over the repulsive one.

There are three remarks to be made about these forces. First, the loss of heat by the mass, by radiation, etc., would diminish the repulsive force on A, and thereby cause contraction. Secondly, as the sphere contracts the gas will become more dense, and this will increase the repulsive force. Thirdly, the attractive force has something else to do than merely to resist the repulsive force; for the sphere is rotating, and therefore every particle of it tends to move onward, at a uniform rate, in a straight line, and therefore the sphere would be broken up and scattered through space. Part of the attractive force, therefore, is used up in bending the paths of the particles round, so as to keep them all together, and if the particles were free, under the action of this force any one of them, as A, would move in a plane passing through its direction of motion at any moment, and C, the centre of the sphere, that is, of the attractive force.

But A does not move so; since the sphere rotates about the axis PP′, A moves round on a parallel of latitude A B; hence we see there is a third force acting on A, viz. the resultant of the lateral resistances of the particles surrounding A, which keeps the particle A in its place on the sphere, so that as the sphere contracts A is always on the same latitude. As the particle, if free, would begin to move southwards from its position at A, describing as it would an orbit in a plane through C, it follows that this resultant of the lateral resistances must act northwards to counteract this.

Now consider the force along A C, this is the difference between the attractive and repulsive forces; let us represent it by A D; let the axis

PP′, meet the plane in which A moves in C′, join A C′, and from D draw D E perpendicular to this plane, E will then lie in A C′, and D E will be parallel to PP′; complete the parallelogram A E D F. Then by the parallelogram of forces, the force represented by A D, that is, the resultant of the attractive and repulsive forces, may be replaced by two forces represented respectively by A E and A F. Now A F is lost in counteracting the resultant lateral resistance on A, all but a small remainder which will produce a slow southward movement of A, corresponding to, or rather identical with, the slow southward movement, due to that slow movement of A along A C, which is produced by or produces the slow contraction of the sphere; the force A E has no resistance to overcome, and may therefore be considered as the force which maintains A in the orbit in which it moves. This orbit will be a spiral, very nearly a circle, and the excess of force over that needed to hold A in a circular orbit will produce a slow inward motion of A, which is identical with the slow approach to the axis due to that slow movement of A along A C.

Now we may regard the excess of A F over the resultant lateral resistance, as producing a change of the position of the plane in which A moves; this change causes a change in C′, but inasmuch as the axis is perpendicular to that plane, the change in the position of C′ is a translation perpendicular to the plane; moreover, A F acts upon A perpendicular to the plane;—hence, neither the forces other than A E acting on A, nor the changes produced by those forces, have any resolved part in the plane; and therefore the motion of A produced in the plane by A E, is exactly the same as if the plane were at rest.

Now consider the motion of A in its plane. It is acted on by a force continually directed towards C′, and therefore, as Newton proved, it will describe equal areas in equal times about that point. Now, as the sphere slowly contracts, the path of A will be a spiral, any one revolution of which will be very nearly a circle; and since the particle A is kept "in its place on the sphere" by the particles round it, the radius of any one revolution of its spiral path will bear a constant ratio to the radius of the sphere at the time when A is describing that revolution. Suppose, then, that when the particle is moving in the outer circle (Fig. 30), it moves from A to B in a given time—say a year; when the sphere has shrunk to only half its size, the particle will be moving in the smaller circle of half the radius of its former path; and in this new path it must move from *a* to *b* in a year, where *a b* is such a distance that the area *a* C *b* is equal to the area A C B. But the area of a triangle, two of whose sides are radii of a circle, and the third a part of the circumference, is measured by half the product of the radius, and the arc; therefore, since *a* C is only half A C, *a b* must be double of A B, in order to make the areas *a* C *b* and A C B equal; therefore, the particle is moving twice as fast as before, but since *a b* is double of A B, and *a* C only half A C, the angle *a* C *b* is four times as great as the angle A C B, and the sphere is spinning four times as fast; and this may be proved generally, that the velocity of every particle of the sphere varies inversely as its radius,

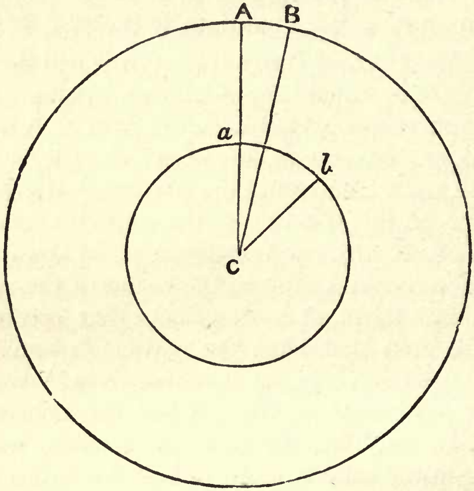

FIG. 30

and the rate at which the sphere is spinning varies inversely as the square of the radius.

Now we know, from elementary kinetics, that the force necessary to hold a body moving with velocity v in a circular orbit of radius r is proportional to $\frac{v^2}{r}$; but we have just seen that v is proportional to $\frac{1}{r}$;

therefore the force needed to keep any particle of the sphere in its orbit, is proportional to $\frac{1}{r^3}$. Now, the only force on any particle A, tending to hold it in its orbit, is the force of gravitation, due to the attraction of all the other particles, and this is proportional to $\frac{1}{r^2}$. Hence, as the sphere contracts, the force needed to hold the particle in its orbit increases at a more rapid rate than the attractive force, and would, after a time, catch it up, and then the whole attractive force would be needed to hold the sphere together, and no further contraction could take place.

But contraction will cease long before that; for though the attractive force of gravitation is the only one tending to produce contraction, the necessity for force to bend the paths of the particles of the rotating sphere so as to hold them all together, is not the only thing that resists the attracting force producing condensation; there is the repulsive force besides, and there is yet another reason.

Consider for what particles of the sphere the whole of the attractive force would first be needed to keep them on the sphere. The force required is $\frac{v^2}{r}$, and in a rotating sphere, v for any particle is proportional to the circle it describes about the axis, since each of the particles describes its circle in the same time; but the circle is proportional to r, where r is the distance from the axis; therefore $\frac{v^2}{r}$ is proportional to r, and this is greatest round the equator; hence the particles round the equator will first cease to approach the centre. But this effect on the particles will be increased by the fact that the sphere as it contracts will lose its spherical form, and bulge out round the equator; hence, $\frac{v^2}{r}$ being proportioned to r, will be greater at the equator than if the mass were a true sphere.

The general result we have arrived at, then, is, that as the spherical mass of gas contracts, there will come a time when the particles ranged round the equator will approach the centre no more, while all the other particles will continue to do so for some time after. But the equatorial ring thus torn off will not be composed of a mere string of the ultimate atoms of the gas; for the gas will probably not be perfectly homogeneous, and if any portion of it near the edge were of greater density than the rest, all the particles of that portion would tend to keep together owing to the increased gravitation of its parts; it might be also that as the sphere condensed, a force of cohesion would spring up between its parts, and hence the particles round the equator would be dragged inwards, until the difference between the attractive force and the repulsive force would, for a considerable portion of matter round the equator, be wholly needed to keep this matter in its place on the sphere; after this the central portion would contract, leaving a ring of matter torn off from it, which continues to approach the axis no more.

If the portion thus torn off be a small irregular dense piece, it will move about the central mass just as it did when it was a part of it, that is, in a circle whose centre is the centre of the central mass; or nearly so, for since it was dragged inwards a little, after the force tending to produce contraction was wholly needed to keep it on the sphere, it will be projected with a velocity rather greater than that needed to make it revolve in a circle, hence it becomes a planet moving in a *slightly* eccentric orbit.

If the portion torn off be a ring, it is probable that it will not be perfectly even, since the sphere is not perfectly homogeneous; the central portion will reassume a spherical form, and continue to condense until, for another ring round its equator, the whole force tending to produce contraction is needed to keep it on the oblate spheroid, when another ring will be thrown off, and so on.

Now let us consider the first ring. Soon after it has been thrown off, the whole matter is distributed as in Fig. 31, where, however, the size of the ring is much exaggerated in proportion to that of the central mass; the portions of greatest thickness would then attract to themselves the thinner portions, and the ring would break up into pieces. If, as is most probable, some of these were of considerable relative size, they would so perturb the relatively smaller pieces, and one another, as that in time they would all gather up into one body, which would then assume a spherical form under the gravitation of its parts, and as in the case of a single piece torn off, would become a planet moving in a slightly eccentric orbit.

The history of the formation of the planet shows that it will move in the plane of the original

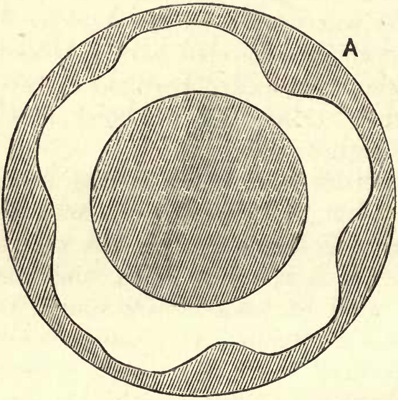

FIG. 31.

equator of oblate spherical mass; but it will probably not do so exactly. Let us consider (Fig. 32) a section of the whole mass through its axis and through one of the thick parts of the ring, just when the ring is being torn off; the line along which it will be torn will probably be irregular, and hence the centre of gravity of the thick part will probably not be exactly in the plane of the equator; and since when it has broken up the piece will move so that its centre of gravity moves in a plane passing through the centre of the central mass, it follows that the resulting planet will move in a plane *nearly*, *but not quite* the same as that of the equator.

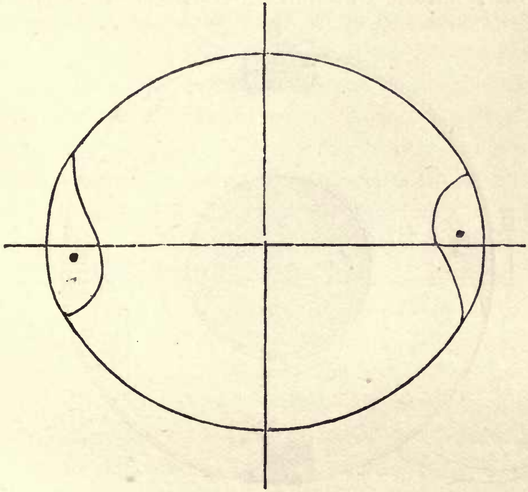

FIG. 32

Let us now consider the motion of this planet about its centre of gravity. The original ring out of which it has been formed—and this argument applies equally well to the case of a single piece torn off—moved about the central mass so that its inner parts described their orbit in the same time as its outer parts. This motion will be shared by the pieces when they have been broken off and agglomerated into a single planet, since there is no reason to the contrary. Suppose, then (Fig. 33), we have a piece of the ring at A; when it has got a quarter of the way round its orbit it will be in the position B, when it is half way round it will be in the position C, when three quarters of the way

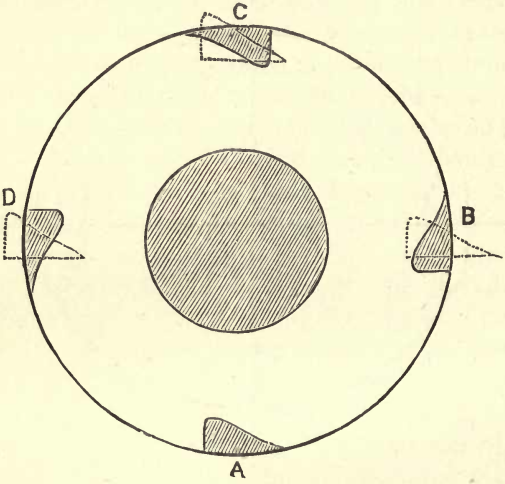

FIG. 33.

round in the position D. This shows that it has a motion of rotation, for if it had not, any line in it would keep in the same direction, and it would be at B, C, and D respectively in the positions of the dotted lines. It follows, then, that the planet will at first rotate so as to keep always the same face towards the central mass, that is, it will rotate at the same rate as the central mass, and in the same direction.

But as the planet contracts it will repeat the history of the central mass. It will spin faster and faster; it will throw off rings which will become satellites; and it will continue to do this until it has at last assumed the solid form.

Let us now see how this theory is verified by the observed facts of the solar system. It obviously accounts for the more conspicuous peculiarities, viz. that the orbits of all the planets and satellites are very nearly circles, and nearly in the same plane, and that they rotate on their axes, and revolve in their orbits, in the same direction. But there are other facts which it will explain. We have seen that the probability is that the ring that is torn off will break up, and the parts agglomerate into one planet; accordingly we find that nearly all the planets and all the satellites move in quite different orbits. But it might be that the ring breaks up into a multitude of very small pieces, whose masses would not sufficiently perturb each other to agglomerate into one planet; in this case we should have a number of very small planets moving nearly in the same orbit; this, however, would not be very likely to happen, accordingly we find only one such case in the solar system—that of the asteroids. Again, it might possibly happen that the ring was so evenly torn off that it never broke up at all, but continued to contract as a ring; we have one case of this in the solar system—the ring of Saturn.

In this case, however, owing to the flatness of the ring, we must suppose that it consists of a number of hoop-like rings, successively thrown off; and, owing to its stability, that it broke up into a multitude of minute pieces just before it solidified.

If the stars and the solar system were thus condensed out of a gas, the enormous distances which separate them compared to their masses would be accounted for; and, inasmuch as the outer rings were thrown off in a more elementary stage of condensation, the planets which were formed of them would be able to contract much farther before becoming solidified, and therefore we should expect the remoter planets to have the greatest number of satellites. This is actually the case. Mercury and Venus have no satellites, the Earth has 1; Mars 2; Jupiter 4; Saturn 8; and Uranus and Neptune have several, and may have some which have not yet been discovered owing to the difficulty of observing them.

This theory will also give a general suggestion of the explanation of the peculiar motions of comets. It seems possible that comets may have been formed of portions of the original nebula which were left nearly balanced between two condensing centres. These would be pulled nearly straight down to the centre, whose attraction ultimately overcame, and would move in very elongated orbits, and there would obviously be no reason why they should move in the plane of rotation of the spherical mass round which they revolve.

We must at the end of this sketch of the Nebular Hypothesis caution the reader, as we did at the beginning, that the reasoning we have just been following is by no means rigorous; in fact, strong objections may be raised to almost every step of the argument, though none of them seem to be absolutely fatal; on the other hand, if we trace back the history of the solar system from the state in which we see it now, supposing the same physical laws to have held good in the past, as those we find standing at present, we are led inevitably to the hypothesis we have been considering, or to some other theory differing but little from it.

We will now pass on to consider an allied theory, which is of much greater certainty, and which rests on strict mathematical reasoning.

So far—except as to rotation—we have treated the planets and satellites as points; but the latest work in astronomy takes account of the influence on this theory of certain effects produced in the masses of the planets and satellites. We have no space here to give much more than the results of this work, which has been carried out by Mr. G. H. Darwin in a series of papers in the Philosophical Transactions since 1877.

We must suppose that the bodies of the solar system passed from the gaseous into the liquid state, and thence into the solid. When they were in either of the two former states, a tide would be raised in their mass exactly as tides are raised on the sea. Suppose a liquid planet has a tide raised in it by a satellite; if the planet rotated in the same time as its satellite revolves, the only effect would be that it would be elongated in the direction of the satellite; but if the times of rotation of the planet and revolution of the satellite were different, the line of elongation would not rotate with the planet itself, and the planet would be constantly *kneaded* by the attraction of its satellite into a different form to that which it at any moment occupies. The friction of its parts resists this, and consequently resists the causes of it.

Now, in the case when the revolution of the satellite is in the same direction as, but slower than, the rotation of the planet, which is the case of the earth and moon, the cause of this "kneading" into the changing tide, is either, or to speak more strictly, *both* the relative quickness of the earth's rotation, and the relative slowness of the moon's revolution; and consequently both these will be resisted by the friction of the parts distorted to produce the tide. The earth's rotation will therefore continually diminish, and what is not so easy to see, the moon's rate of motion will be accelerated, the effect of this will be, that she will be bent round by the earth's attraction less, hence her orbit will become continually larger, and her mean distance will be increased; the effect of this by Kepler's third law will be that her velocity will be diminished; but the rate of her revolution will ultimately increase relatively to the earth's rotation, and after many millions of years, the two will be equal; the earth will then always turn the same face to the moon, and the day and month will, Mr. Darwin calculates, be each about 1400 hours in length. The tide raised in the moon by the earth when she was molten, would be much greater than that raised in the earth by the moon; and hence the reduction of the moon to the condition in which she presents always the same face to the earth has already taken place.

It is important to notice that as the moon's rate of revolution round the earth diminishes, the physical libration first pointed out by Lagrange * will come into play, and the moon's rate of rotation will be diminished at the same rate as her rate of revolution, so that she will continue to present the same face to the earth.

But these causes have been operating in the past. Hence the distance of the earth and moon was formerly much less. About fifty-six million years ago, or at some previous time, they were, so far as this cause is concerned, touching; at the same time it can be calculated that the earth's rotation must have been at the rate of once in some time lying between two and four hours, probably about once every three hours. Kepler's laws tell us that this must have been the rate at which the moon in that position was revolving round the earth; hence earth and moon were then in contact, and revolving round one another as if they were one

* *Vide* p. 254.

rotating piece; what more likely than that the moon was ruptured off from the earth when they were in that position? But can we assign any cause for such a rupture?

At that time, and ever since, a tide was produced in the earth by the sun, which however was, and is, much smaller than the lunar tide; this, if the earth were rotating at the rate of once in three hours would repeat itself every hour and a half; but the earth when fluid, though it had no elasticity, would under the attraction of its parts tend to assume a spherical form, and if displaced from that form would spring back into it again, would overshoot the mark, and then return, and so on vibrating like an elastic solid, and it can be calculated that its time of vibration is about an hour and a half; the tides, therefore, would synchronize with the vibrations they produce, the result will be important as we can see by an illustration.

Suppose we have a heavy weight, such as a cannon ball, hung by a light wire, so as to swing like a pendulum, and vibrate say once every two seconds; if we give it a small blow with a light hammer we shall move it only a little bit, suppose we continue to give it a light blow every second, the first blow will cause it to move a very little; in one second it will be in the position from which it was struck, but moving *in the opposite direction*, but with the same velocity as that with which it was started; the second blow, therefore, given at this moment, a second after the first, will just neutralize the effects of the first, and reduce the pendulum to rest; and in the same way every blow will just neutralize the effect of the immediately preceding one, and the pendulum will scarcely move at all.

The same may be proved to be the case at whatever intervals we strike the pendulum, except in the case when the interval is the same as the period of the pendulum's vibration. In this case, at the moment of the second blow, the pendulum is moving forward in the direction of the blow, and hence will move to a greater distance than when it was struck from rest by the first blow. At the third blow it will be moving still faster, and therefore will go further still, and so on it will increase the amplitude of its swing, until it is swinging violently. Exactly the same must have happened in the case of the earth distorted by the tides raised in it by the sun; each tide will set it vibrating, but since the tides synchronize with the vibrations, the amplitude of the vibrations will increase to such an extent that at last a piece of it may be snapped off, and become the moon.

This will be still further facilitated by the fact that the rate at which the earth was then spinning, once in about three hours, can be shown to be almost sufficient of itself to cause her to fly to pieces; so that a small vibration would be sufficient to snap a piece off.

It appears, therefore, that this "tidal evolution" is the preponderating influence in the formation of the earth; n the case of the other planets, however, Mr. Darwin has calculated that it would not appreciably modify their evolution as given above. One effect, however, is common to all the planets and satellites, each of two bodies that revolve round each other produces a tide in the other, and this tide tends to reduce the rate of rotation of the body in which it exists, to the same magnitude as the rate of revolution of the bodies round each other. It follows, therefore, that the solar tide will tend to drag the rotation of the planets after they rotate at the rate of revolution of their satellites. A discovery has been made just in time to verify this. Mars is the only planet which has reached this stage, and as he is the smallest planet that has satellites, he ought to reach it first. The nearest of his satellites revolves in about seven hours and a half, while the planet itself rotates in about twenty-four hours and a half.

Of the other results Mr. Darwin has arrived at we have no space to treat; it is, however, worth while to mention that the discoveries of Lagrange and Laplace as to the stability of the solar system treated the planets and satellites as material points, and did not take account of tidal friction; it follows that the vaunted stability is after all not true, though the causes which will destroy it will take an incomparably longer time to do so than would the perturbations which were formerly supposed to be doing the work.

One other effect of this retardation of the earth's rotation and the moon's revolution must be noticed.

It is obvious that it has some influence on the explanation of the apparent acceleration of the moon's motion, and therefore that Laplace could not have been right when he completely explained that acceleration by a different cause. The necessity for some additional cause to account completely for the facts had been evident for some years before Mr. Darwin's work was published; for in 1853, Mr. Adams pointed out that Laplace's approximation was not sufficiently accurate, and that, by carrying the approximation further, the acceleration of the moon due to the alteration of the eccentricity of the earth's orbit was only 6″ a century. This result was at first received with incredulity; but a careful investigation showed that Mr. Adams was right. On the other hand, a more careful reconsideration of the evidence from ancient eclipses seemed to show that the apparent acceleration of the moon had been underrated by Laplace, and that 12″ a century was more near the true result.

Now Mr. Darwin's work proves that, owing to tidal friction, the moon's motion is retarded; that retardation, however, is less than the acceleration found by Mr. Adams, and therefore, during the comparatively short time in which the eccentricity of the earth's orbit has been diminishing, the moon's motion has been slowly accelerated. But the apparent acceleration will be much greater than this. For the only way of measuring time hitherto has been by taking the day for unit, but owing to tidal friction, the length of the day is increasing, and moreover, increasing at a greater rate than the month is increasing owing to the same cause; hence, owing to tidal friction alone, the revolution of the moon measured in days, will be accelerated. It is almost impossible to obtain exact numerical results, because these must depend upon the viscosity of the earth, and water surrounding her, and on other causes of which we have very imperfect data; but it is probable that tidal friction, combined with the diminishing of the eccentricity of the earth's orbit, completely explains that slow acceleration of the moon's motion which was first discovered by Halley.

It will be observed, however, that the whole of this theory of the evolution of the solar system depends upon the assumption of the original existence of nebulous matter, and the only direct evidence of this that we have hitherto considered is the doubtful appearance of nebulous stars. But we have a positive proof that some of the nebulæ are in a gaseous state; and this leads us to the third great question that can be asked in astronomy. What are the heavenly bodies made of? The answer to this has only been attempted within the last twenty years and as its great instrument of inquiry is spectrum analysis, an explanation of it belongs rather to the conclusion of a volume on the lives of the physicists than to the present work; but owing to the great importance of its results a brief sketch of it must be given here.

It has already been pointed out that a ray of light in passing from one medium—one sort of transparent stuff—into another will be bent more away from the surface separating the two in the denser medium. In dealing with this subject an instrument called a prism is used; it consists essentially of a piece of some sort of dense transparent stuff (usually glass), two of whose sides are plane surfaces meeting along a straight edge. The view of a prism seen end on is given in Fig. 34, and we there see that if a ray of light R E be allowed to

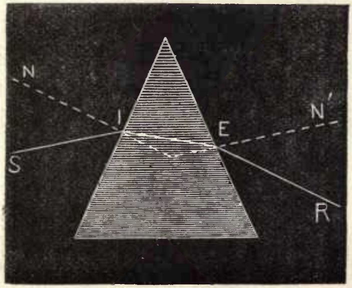

FIG. 34.

fall on it in the proper direction it will in passing through the prism suffer two refractions, one at each of the plane surfaces, and thus will be bent much more than by a single refraction.

Now if different coloured rays be allowed to fall on the same prism, they will be differently bent by it, red being least bent, and other colours in the order of red, orange, yellow, green, blue, indigo, and violet, the last being most bent; this fact was first formally stated by Newton in his "Opticks," where he says, "Lights which differ in colour, differ also in degrees of refrangibility;" though there is no doubt that it was known to Kepler.

For clearness sake, let us suppose we have a ray of sunlight falling through a round hole in a shutter into a dark room; let us pass it through a prism and receive it on a white screen; a round image of the hole in the shutter will appear on the screen at R, O, Y, G, B, I, or V (Fig. 35), according

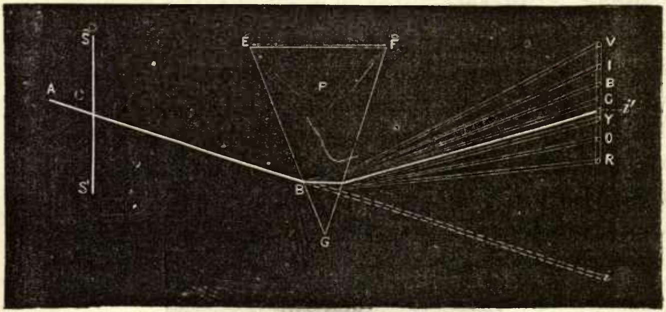

FIG. 35.

as we colour the ray red, orange, yellow, green, blue, indigo, or violet. The question now arises, what will happen if we allow the white uncoloured ray to fall upon the screen? If we try the experiment we find instead of a white image of the hole, a band of colours on the screen stretching from R to V, and containing all these colours, and not only all the seven mentioned above, but every intermediate tint, so that the sides of the band are straight. The band of colours is called the spectrum of the source of the light that produces it—in this case the sun. It seems then that the white light of the sun contains all possible colours mixed together. It is important to notice, however, that if any particular tint, or range of tint, within small limits, be absent, we should probably not be aware of it owing to the overlapping of images of tints on either side of those which are absent.

In 1802 a small but most important alteration was made in the experiment as conducted by Newton. In that year Dr. Wollaston used a narrow slit parallel to the edge of the prism, instead of a round hole to admit the light into the dark room. The spectrum being formed of images of the slit on the screen, there would be much less overlapping than in the former arrangement, and hence it would be easier to observe if any colours were absent. Dr. Wollaston was able to observe that a large number of colours were absent from sun-light, showing themselves in no less than 576 dark lines crossing the solar spectrum. Since then more accurate experiments have revealed many thousands of these lines in the solar spectrum. The question now arises, what is the cause of them?

Since all coloured transparent media produce the colouring of the light that passes through them, only by stopping all or most of the colours except the one that passes through them, it might be that the air stops certain colours and thus produces these dark lines. But if so we ought to observe them in the spectra of the fixed stars and nebulæ; but Fraunhofer, in 1814, observed that the lines are not at all the same for these bodies as for the sun. For the same reason the cause does not exist in the interplanetary spaces; but since we do observe exactly the same lines in the spectra of the moon and planets, which merely reflect sunlight, we are driven to believe that the cause, whatever it is, exists in the immediate neighbourhood of the sun himself. Similarly, the causes of the different dark lines in the spectra of the stars and nebulæ must exist in their immediate neighbourhood.

Soon after the discovery of these lines, Fraunhofer found that a white hot or incandescent solid or liquid will give a continuous spectrum crossed by certain faint dark lines, which are the same on all spectra, and are due to the passage of the light through the air. On the other hand, an incandescent gas gives a spectrum consisting only of several distinct bright lines; several distinct images of the slit of different colours appear on the screen. This shows that the light of an incandescent solid consists of all possible colours, with no gaps, of an incandescent gas, of only certain particular colours.

Later on, Kirchhoff made a map of the spectra of some of the gases, by passing the beam of light from the gas along a parallel direction to that of a beam of sunlight, and through the same slit and prism, so that the two spectra might appear side by side on the screen, and then observing between which two of the dark lines of the solar spectrum each of the bright lines of the gaseous spectrum lay. He found in particular that the spectrum of incandescent iron vapour consists of four hundred and sixty bright lines, every one of which was exactly opposite to a dark line on the solar spectrum; and he calculated the odds against this being accidental to be about one million billions to one. The question then arose, what is the connection between them?

He found later on, that if light from an incandescent solid or liquid be passed through a gas at a lower degree of incandescence, certain dark lines will appear on its otherwise continuous spectrum, in exactly the places where bright lines would appear if the spectrum of the gas were being examined; and this he verified by increasing the incandescence of the gas, until it was greater than that of the solid; during this process the dark lines were observed to become fainter, until they for an instant disappeared and then reappeared as bright lines. It follows, therefore, that a gas will stop just those colours which it will itself give out when incandescent.

And this also should follow from the theory of light, as we can explain by an illustration. If we sing, or otherwise sound a note into a piano, and then suddenly stop, we shall hear the same note given back to us faintly by the piano; the reason is that sound consists of waves or pulses moving through the air and producing vibrations in it; each string in the piano is tuned to vibrate at a particular rate, and every string will be displaced somewhat by the vibrations of the air, caused by the sounding of the note; but just as with the pendulum, in our explanation of the origin of the moon, the only string which will have any extensive vibrations produced in it, will be that one which is tuned to vibrate at the same rate as the air, that is, to give out the particular note which is being sounded. Now, suppose we have a multitude of strings all tuned to vibrate at the same rate, to give out, that is, the same note, completely surrounding an organ, then if that note be sounded on the organ, the whole energy of the air set vibrating inside the strings, might be taken up in setting the strings vibrating, but not enough to give out the note again, so that, outside the strings the note would not be heard; while all other notes, which the strings would not themselves give out when plucked, would pass through unaffected.

Exactly the same takes place with light. Light consists of waves of a certain sort, in a medium which is supposed to pervade all space; a gas is "tuned," so to speak, to vibrate at certain particular rates, to give out, that is, certain particular colours; it will, therefore, just as the strings, stop just those colours which it would itself give out when incandescent.

Now there is no other cause known which will stop particular colours, than the passage through a gas, and no two gases known have the same spectrum; and no two gases known, with the possible exception of two or three, have any one line common to both their spectra; hence, when we find any number of dark lines on an otherwise continuous spectrum, exactly corresponding with all the lines of the spectrum of a particular gas, we may be certain that the source of light is an incandescent solid or liquid, and that the light has somewhere passed through that gas.

We have given reasons which show that the cause of the "iron lines" in the solar spectrum, must be near the sun; knowing the sun to be intensely hot, it seems the only possible explanation that iron exists in the sun in a gaseous form.

Nearly all the lines in all the spectra of the heavenly bodies have been recognized as belonging to gases with which we are familiar on the earth; hence, all the universe is made of the same sort of stuff, which is to some extent a verification of the nebular hypothesis; but the most complete verification is given by the fact that some of the nebulæ have gaseous spectra; and even the great nebula in Orion, *which was resolved* by Lord Rosse's telescope, gives a gaseous spectrum, so that here we have a case in which the nebula is still gaseous, though it has begun to condense round distinct centres.

If the moon and planets have an atmosphere we ought to observe its lines on their spectra, otherwise the same as that of the sun; if that atmosphere be the same as the air, the air lines ought to be intensified. In this way we can tell that the moon has no atmosphere, while each of the superior planets has an atmosphere similar to the air, and in Mars, Jupiter, and Saturn, aqueous vapour is conspicuous. The spectra of the inferior planets are difficult to observe.

One of the most striking operations of spectrum analysis is the application of it, devised by Dr. Huggins, to the determination of the rate of motion of the stars, to or from the solar system. To explain this we will again use an illustration from sound. It is a familiar fact that if a locomotive engine pass us whistling, the pitch of the note of the whistle drops at the instant it passes us. The explanation is as follows. Suppose the note given out be that due to five hundred vibrations a second (the quicker the vibrations the higher the note), then, since sound travels, roughly speaking, one thousand feet in a second, the "wave length," or distance between any two consecutive pulses will be two feet, but if the source of sound be moving towards us the note will be higher. For if the whistle be at A (Fig. 36), when the first pulse is given out, when the first pulse has travelled to B,

FIG. 36.

two feet from A, the second pulse is just being given out by the whistle, but the whistle has now moved to A′, and therefore the second pulse will be given out less than two feet from the first, similarly the distance between the second and third pulses will be shortened, and all the wave-lengths will be less than two feet, but the rate of motion of the waves of sound through the air will not be affected by the motion of the whistle, so that more than five hundred will meet the ear in a second, and the note will be heard of a higher pitch than it is given out by the whistle. Similarly, when it is moving away, the note will be lowered.

Now, light of any colour is due to quicker vibrations than light of a colour nearer the red end of the spectrum; and Dr. Huggins observed, that in the spectra of some of the stars, the dark lines were at exactly the distance apart that they would be if they were produced by known gases, but that they were all displaced out of their true positions on the spectrum. He therefore was driven to believe that when the lines are displaced towards the blue end of the spectrum the star is moving towards, when towards the red end it is moving from us; knowing the wave-length of light of every colour, and knowing the velocity of light, he was then able to estimate by measuring the displacement the rate of motion of the star.

Of all the heavenly bodies, that of whose constitution spectrum analysis has taught us most, and that which is the most important to us, being the source of all the heat and light and life of our system, is the sun. During a total eclipse we can see much of him which is ordinarily obscured by his glare. He appears to consist of several concentric spheres of different sorts of matter. Outside all is the zodiacal light which, on the nebular hypothesis, consists of uncondensed nebulous matter. Inside this, but surrounding the sun to a distance about equal to his diameter, and with streamers issuing from it to a much greater distance, is a bright glare called the "corona." Inside this again, and close round the sun, is a rose-coloured envelope called the "chromosphere," from which prominences issue occasionally to a height of more than a third of the sun's diameter, and inside all is the bright surface of the sun which is ordinarily visible, and which is called the "photosphere;" on this are dark spots which look like holes opening into unknown depths.

Spectrum analysis tells us that the zodiacal light is reflected sunlight; that the corona shines mainly with reflected sunlight, but that it also contains a self-luminous unknown gas; that the chromosphere with its prominences consists almost entirely of incandescent hydrogen, though occasionally other gases appear in it; the photosphere gives the ordinary solar spectrum, and therefore must consist of incandescent solid or liquid, or gas at an intensely great pressure. The most probable hypothesis seems to be that it consists of clouds of condensed "droplets" of the vapours that surround him exactly as water clouds are formed in our air.

The mean density of the sun being small, only about a quarter that of the earth, and his heat being intense, we are led to believe that he must consist wholly of gas, his heat being kept up by that gradual contraction which has evolved the planets, and which is still going on. It can be calculated that in order to produce the expenditure of heat which would agree with meteorological observations, the sun would only have to contract at so slow a rate that the most accurate telescopes we now possess would only just detect a diminution of diameter after three thousand years.

Dr. Huggins's method tells us that there is a downward current at a dark spot, and an upward current at those bright streaks or spots which usually surround a dark spot. It seems, therefore, that these spots are due to convection currents; that the greater density and coolness of the downward current produces less light and obstructs more, and possibly pierces the glowing clouds which form the photosphere, and to which the light of the sun is mainly due.

There are many other phenomena connected with sun spots which are in themselves sufficiently striking, but for which no explanation so much as plausible has yet been suggested. It is observed, for instance, that the rate of rotation of the sun, as revealed by the motion of sun spots, decreases from the equator to the poles. But the most remarkable fact connected with them was found accidentally by Schwabe, who made a complete series of observations of the number and extent of them for twenty-five years, which he published in 1851, and who therein, as he himself says, went "like Saul to seek his father's asses, and found a kingdom." He found that his observations showed the extent and frequency of sun spots to recur in periods of between ten and eleven years, and this completely agrees with all previous and subsequent observations that have been made. But a still more remarkable fact is that the extent and frequency of magnetic storms on the earth's surface occur in periods of the same duration, and their maxima and minima synchronise. An appearance of the aurora borealis is invariably accompanied by a violent magnetic storm, and generally *vice versâ*; observations on the emission of the sun's heat agree in showing a similar periodic variation of it; but, strange to say, they do not agree as to whether the maximum of heat is emitted when there is a maximum of spots, or when there is a minimum of spots.

The observed connection between all these facts is indubitable, but the chain of cause and effect by which they are connected is one of those "infinite treasures" of knowledge which still lie, for us, undiscovered in the ocean of the future.

We have now reached the end of our sketch of the great story of the growth of astronomical knowledge; and we have left the reader looking onward into the future where we can, even now, catch glimpses of knowledge that shall one day be clear. If the study of the law of the stars still seems a dry mechanical thing, robbing the heavens of their beauty, and life of its poetry, it is the fault of us who have told the story, and not of the study itself.

These old heroes of science fought not less nobly because—

> "Their battles cost no tears, no land distressed;"

and by their lives they have taught us that genius has even more to do with the heart than the head. Their labours, it is true, have overturned men's old notions. The earth, our home, now no longer sits enthroned as the centre of the universe, but has been degraded into a minute attendant on one of the lesser stars. Yet her absolute importance has not been changed. To us she is as great as she was to our fathers; and human hopes, and fears, and joys, and sorrows, will stretch upward into the darkness, and need and find a Comforter as well from this little planet as when men thought her the centre of the seven heavens.

It is true there have been, and still are, some cold-hearted men of science who will deny this, who proclaim, and rejoice to proclaim that science is atheistic; who, because they believe our God has left the mountain tops, would drive Him beyond the stars. And there are still some timid religious people, who fear that the unfettered use of the powers God has given us, may stay us from putting our trust in Him, and may even lead us to disbelieve His existence. This surely cannot be. But there is one evil thing which is sometimes perversely confused with religion, with which science has no truce, and with which the religion of Christ has no more truce than science. That thievish desire for a dominion over his fellows that belongs to no man, and which is the poisonous root of all oppression, has prompted some men to pretend to divine authority over thought. This tyranny science will not acknowledge. She will only treat with the reason of each man, as Christ only appealed to the conscience of each man. She stands too humble before truth to trust it to the bonds of any human opinion; and, as we have seen, the labours of these heroes of science have exposed the futility, and decreed the overthrow of "authority." Like Dante, who called into being a language while he thought he was but writing a poem, they thought they were but finding the law of the stars, while they were freeing thought for all time.

But it may be there are some of our readers who cannot look full upon these latest teachings of science—the evolution of the planets—without finding the foundations of their faith somewhat shaken. It may be there are still some who will make their own literal interpretation of the words of Scripture the test and measure of divine truth, who choose to believe that God works like a carpenter and joiner, piecing together an article that time can only spoil, creating it at its best, and leaving it to gradual deterioration and slow decay; and who will not understand that as the ages roll on, "He discovereth deep things out of darkness, and bringeth out to light the shadow of death." To these we can but commend the advice of grand old John Kepler. "If anyone," he says, "be too dull to comprehend the science of astronomy, or too feeble-minded to believe in (its teachings) * without prejudice to his piety, my advice to such a one is, that he should quit the astronomical schools, and condemning, if he has a mind, any or all of the theories of philosophers, let him look to his own affairs, and leaving this worldly travail, let him go home and plough his fields; and as often as he lifts up to this goodly heaven those eyes with which alone he is able to see, let him pour out his heart in praises and thanksgiving to God the creator; and let him not fear but he is offering a worship not less acceptable than his to whom God has granted to see yet more clearly with the eyes of his mind, and who both can and will praise his God for what he has so discovered."

* This passage is taken from the "Commentaries on the motions of Mars;" and the words "its teachings," in brackets in our quotation, are replaced in the original by the word "Copernik," his discoveries being at that time those which were regarded as "unorthodox."

THE END.

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